THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[SECOND SERIES.]

ART. XXX.—*On American Devonian;* (in a letter to the Editors from J. W. DAWSON, Principal of McGill University).

Gentlemen:—In a communication from Professor Winchell, in your January number, I observe that some American geologists are inclined to refer certain rocks, hitherto regarded as Upper Devonian, to the Carboniferous period. Will you permit me to state some facts, derived from the study of fossil plants, which seem to me to militate against this view, at least in so far as Eastern America is concerned.

In my investigations of the Devonian flora of Eastern America, carried on for several years past, and the latest results of which are published in the number of the Journal of the Geological Society of London for November, 1862, I have described or identified sixty-nine species of land plants from Devonian beds; and of these only 10 or 12 are even probably Carboniferous species. Of thirteen species from the Chemung group of New York,[1] kindly communicated to me by Prof. Hall, not one is known as Carboniferous. All are of Devonian forms, and the most abundant species are also found in the undoubtedly Devonian Gaspé sandstones, as well as at Perry in Maine, in both of which localities the flora is quite distinct from that of even the lowest Carboniferous beds, ("Sub-Carboniferous" of some authors). At St. John, New Brunswick, where, in beds which I believe to belong to the Upper Devonian, there is a more abundant flora than at the other places mentioned, a larger number,

[1] Including the beds formerly incorrectly referred to the Catskill group.

but still only a small proportion of the species, are probably Carboniferous. In Pennsylvania, in so far as I can judge from the statements of Mr. Lesquereux and the figures given by Prof. Rogers, the flora of the "Vergent" and "Ponent" series appears to be of similar character with that of the Chemung of New York.

In Europe the observed facts are not dissimilar from those above stated. Gœppert enumerates fifty-five species as known to him in the Upper Devonian, and of these only six seem to be Lower Carboniferous. Of forty-six species from the Cypridina Shales of Thuringia, described by Unger, only four are Carboniferous. The scanty flora of the Devonian of Scotland and Ireland, described by several British authors, appears to be equally distinct from that of the Carboniferous rocks, while it closely resembles that of our American Upper Devonian. It is also to be observed that several generic and sub-generic forms of the Devonian are wanting in the richer flora of the overlying system.

In the Carboniferous system, while it is true that there are somewhat different assemblages of plants in the Lower, Middle, and Upper members; and that, within these, there are minor differences, arising probably from local causes affecting the distribution of species, and also from the greater or less amount of driftage, and the greater or less coarseness of the sediments, there is a grand unity of the fossil flora throughout. Even in the lowest Carboniferous beds, at least in Eastern America, the genera and most of the species are identical with those of the middle Coal Measures, separated from these lower beds by the Marine Limestones and the Millstone Grit. On the other hand so soon as we descend to the Devonian, we find some new genera and a distinct assemblage of species.

The only apparently exceptional case known to me, and this may have some connection with the facts stated by Prof. Winchell, is that of certain beds at Akron and Richfield, Ohio, which have, I believe, been regarded as equivalent to the Upper Devonian of New York. In a small collection from these places, shown to me by Prof. Hall, I observed two species which I regard as identical with Lower Carboniferous forms, while the other species present, though some of them have a Devonian aspect, are not certainly identical with any of the New York or Gaspé species.

It may very probably be the case that, in the Palæozoic period, the range in time of marine forms exceeded that of terrestrial plant life; but it would surely be an anomaly to have a system of rocks including one flora and a part of another almost entirely distinct, and characteristic of another period. I do not however suppose that there is everywhere so great a gap between the floras of the Devonian and Carboniferous periods as that which

appears in Eastern America. Such gaps are usually local and bridged over somewhere. In the West there may be a transition, as would indeed seem probable from the Ohio plants mentioned above, in connection with the peculiarities of the physical geology; but in this case I should not suppose these beds of passage to be precisely equivalent in age to the Chemung group, but rather to be newer, and possibly wanting or represented by barren deposits in New York.

If such intermediate or passage beds exist in the West, and if their plants have not been already collected and studied by Dr. Newberry or Mr. Lesquereux, it would be very important that attention should be devoted to them, and that they should be carefully compared with the species of the two floras which they may be supposed to connect. I may add that, for this purpose, the most unpromising fragments, especially if capable of showing structure under the microscope, would be of some service; as the characters of the Devonian species have often to be gathered from remains which would scarcely be deemed worthy of the attention of collectors in the rich beds of the Coal Measures.

McGill University, Montreal, Feb. 24, 1863.

ART. XXXI.—*On the Flora of the Devonian Period in Northeastern America;* by J. W. DAWSON, LL.D., F.R.S., Principal of McGill University, Montreal.[1]

[This paper by Prof. Dawson is the one alluded to in his previous communication. The 2d part containing descriptions of species is omitted.]

The existence of several species of land-plants in the Devonian rocks of New York and Pennsylvania was ascertained many years ago by the Geological Surveys of those States, and several of these plants have been described and figured in their Reports.[2] In Canada, Sir W. E. Logan had ascertained, as early as 1843, the presence of an abundant, though apparently monotonous and simple, flora in the Devonian strata of Gaspé; but it was not until 1859 that these plants were described by the author in the 'Proceedings' of this Society.[3] More recently Messrs. Matthew and Hartt, two young geologists of St. John, New Brunswick, have found a rich and interesting flora in the semi-metamorphic beds in the vicinity of that city, in which a few fossil plants had previously been observed by Dr. Gesner, Dr.

[1] Copied from the *Quarterly Journal of the Geological Society*, Nov., 1861.

[2] Hall and Vanuxem, Reports on the Geology of New York; Rogers, Report on Pennsylvania.

[3] *Quart. Jour. Geol. Soc. Lond.*, XV, 477.

Robb, and Mr. Bennett of St. John; but they had not been figured or described. These plants were described in the *Canadian Naturalist*,[4] together with some additional species, of the same age, found at Perry, in the State of Maine, and preserved in the collection of the Natural History Society of Portland. The whole of the plants thus described, I summed up in the paper last mentioned as consisting of 21 species, belonging to 16 genera, exclusive of genera like *Sternbergia* and *Lepidostrobus*, which represent parts of plants only.

In the past summer I visited St. John; and, in company with Messrs. Matthew and Hartt, explored the localities of the plants previously discovered, and examined the large collections which had been formed by those gentlemen since the publication of my previous paper. The material thus obtained proving unexpectedly copious and interesting, I was desirous of having opportunities of fuller comparison with the Devonian Flora of New York State; and, on application to Prof. Hall, that gentleman, with consent of the Regents of the University of New York, kindly placed in my hands the whole of his collections, embracing many new and remarkable forms. Prof. C. H. Hitchcock, State geologist of Maine, had in the meantime further explored the deposits at Perry, and has communicated to me three new species discovered by him. The whole of these collections, amounting in all to more than sixty species, constitute an addition to the Devonian Flora equal in importance to all the plants previously obtained from rocks of this age, and establish for some of the species a very extensive distribution both geologically and geographically; they allow, also, more satisfactory comparisons than were heretofore practicable to be instituted between the Devonian Flora and that of the Carboniferous period.

I shall first shortly notice the geological character of the localities, with lists of the fossils found in each, and shall then proceed to describe the new species.

I. Notices of the Localities of the Devonian Plants.

1. *State of New York.*—The geology of this State has been so fully illustrated by Prof. Hall and his colleagues, and the parallelism of its formations with those of Europe has been so extensively made known by Murchison and others, that it is only necessary for me to state that the fossils entrusted to me by Prof. Hall range from the Marcellus Shale to the Catskill group inclusive, and thus belong to the Middle and Upper Devonian of British geologists. The plants are distributed in the subdivisions of these groups as follows:—

[4] Vol. vii, May, 1861.

UPPER DEVONIAN.

Catskill Group.[5]

Aporoxylon.
Sigillaria Simplicitas *Vanuxem.*
Lepidodendron Gaspianum *Dawson.*
Psilophyton princeps *Dawson.*
Cyclopteris Jacksonii *Dawson.*
Rhachiopteris punctata, sp. nov.
—— cyclopteroides, sp. nov.

Chemung Group.

Sigillaria Vanuxemii *Gœppert.*
Syringodendron gracile, sp. nov.
Stigmaria exigua, sp. nov.
Lepidodendron Chemungense *Hall.*
—— corrugatum *Dawson.*
Lycopodites Vanuxemi, sp. nov.
Cyclopteris Halliana *Gœppert.*
Psilophyton princeps *Dawson.*
Acanthophyton spinosum, sp. nov.
Rhachiopteris striata, sp. nov.

MIDDLE DEVONIAN.

Hamilton Group.

Syringoxylon mirabile, sp. nov.
Dadoxylon Hallii, sp. nov.
Aporoxylon.
Sigillaria.
Didymophyllum reniforme, sp. nov.
Calamites Transitionis (?) *Gœppert.*
—— inornatus, sp. nov.
Lepidodendron Gaspianum *Dawson.*
—— corrugatum *Dawson.*
Psilophyton princeps *Dawson.*
Cordaites Robbii (?) *Dawson.*
——, sp. nov.
—— angustifolia *Dawson.*
Cyclopteris incerta, sp. nov.
Rhachiopteris striata, sp. nov.
—— tenuistriata, sp. nov.
—— pinnata, sp. nov.

2. *Maine.*—The only locality in this State that has hitherto afforded fossil plants is Perry, near Eastport, in the eastern part of the State. The plant-bearing rocks are grey sandstones, resembling those of Gaspé, and associated with red conglomerate and trappean or tufaceous rocks, which, according to the recent observations of Prof. C. H. Hitchcock,[6] rest unconformably on shales or slates holding Upper Silurian fossils.[7] I have little doubt that these beds at Perry are a continuation of part of the series observed at St. John, New Brunswick; and it is probable that they are Upper Devonian. The following species occur at this place:[8]—

Lepidodendron Gaspianum *Dawson.*
Lepidostrobus Richardsonii *Dawson.*
—— globosus *Dawson.*
Psilophyton princeps *Dawson.*
Leptophlœum rhombicum, sp. nov.
Megaphyton?
Aporoxylon?
Cyclopteris Jacksoni *Dawson.*
—— Brownii, sp. nov.
Sphenopteris Hitchcockiana, sp. nov.

3. *Canada.*—Devonian beds holding fossil plants occur in Eastern Canada, in Gaspé, and in Western Canada, at Kettle Point, Lake Huron. At the former place there is an extensive series of sandstones and shales, regarded by Sir W. E. Logan as representing the whole of the Devonian series, and containing

[5] Now included in the Chemung.
[6] Report on the Geological Survey of Maine, now in the press.
[7] See also notices by Dr. Jackson and Prof. Rogers in the *Proceedings of the Boston Society of Natural History.*
[8] A few additional species discovered last summer will shortly be described.

plants throughout, but more abundantly in its central portion.[9] At the latter a few plants have been found in shales of Upper Devonian age. The plants found at Gaspé were described in my former paper, and are—

Prototaxites Logani *Dawson.*	Psilophyton robustius *Dawson.*
Lepidodendron Gaspianum *Dawson.*	Selaginites formosus *Dawson.*
Psilophyton princeps *Dawson.*	Cordaites angustifolia *Dawson.*

The plants from Kettle Point, noticed with doubt in my former paper, I may now refer to the following species:—

Sagenaria Veltheimiana *Gœppert.* | Calamites inornatus, sp. nov.

4. *New Brunswick.*—The rocks in the vicinity of the city of St. John, constituting a part of the coast metamorphic series of New Brunswick, have been described in the official reports of Dr. Gesner and Dr. Robb;[10] and additional facts respecting their stratigraphical relations, ascertained by Mr. Matthew, were stated in my paper in the *Canadian Naturalist*, already referred to. The new interest attached to these beds, in consequence of the discovery of their copious fossil flora, induced me to re-examine all the sections, in company with Mr. Matthew, during my late visit; and that gentleman has recently extended the limits of our observations eastward in the direction of Mispec. The results of these observations I shall state in some detail, as the precise age of the St. John series has not until now been determined.

The oldest rocks seen in the vicinity of St. John are the so-called syenites and altered slates in the ridges between the city and the Kennebeckasis River. These rocks are in great part gneissose, and are no doubt altered sediments. They are usually of greenish colors; and in places they contain bands of dark slate and reddish felsite, as well as of gray quartzite. In their upper part they alternate with white and graphitic crystalline limestone, which overlies them in thick beds at M'Closkeney's and Drury's Coves on the Kennebeckasis, and again on the St. John side of an anticlinal formed by the syenitic or gneissose rocks, at the suburb of Portland. These limestones are also well seen in a railway-cutting five miles to the eastward of St. John,[11] and at Lily Lake. Near the Kennebeckasis, they are unconformably overlain by the Lower Carboniferous conglomerate, which is coarse and of a red color, and contains numerous fragments of the limestone.

[9] Reports of the Geological Survey of Canada; paper on the Devonian plants of Gaspé, *Quart. Journ. Geol. Soc. Lond.*, xv.

[10] Gesner's Second and Third Reports on the Geological Survey of New Brunswick; Robb, in Johnston's Report on the Agriculture of New Brunswick.

[11] At this place the limestone is penetrated by a thick vein of graphic granite, holding black tourmaline; and at Drury's cove, not far distant, it contains dykes of dark colored trap.

At Portland, the crystalline limestone appears in a very thick bed, and constitutes the ridge on which stands Fort Howe. Its colors are white and grey, with dark graphitic laminæ; and it contains occasional bands of olive-colored shale. It dips at a very high angle to the southeast. Three beds of impure graphite appear in its upper portion. The highest is about a foot in thickness, and rests on a sort of underclay. The middle bed is thinner and less perfectly exposed. The lower bed, in which a shaft has been sunk, seems to be three or four feet in thickness. It is very earthy and pyritous. The great bed of limestone is seen to rest on flinty slate and syenitic gneiss, beneath which, however, there appears a minor bed of limestone. Above the great limestone are beds of a hard grey metamorphic rock, apparently an indurated volcanic ash, associated with some sandstone; and this is succeeded by the great series of gray, olive, and black shales and flags which underlie the city of St. John. These rocks are well exposed on both sides of Courtney Bay, in the city of St. John, and in Carlton. Though somewhat contorted, they have a general dip to the southeast, at angles of 50° to 70°. In some of the beds there are great numbers of *Lingulæ*, which have not as yet been identified with any described species. There are also trails of worms, and scratches which may have been produced by the feet of Crustaceans or the fins of fishes.

The comparatively coarse shales above described are succeeded by a thick band of black papyraceous shale, much contorted, and with a few thin seams of calcareous matter arranged in the concretionary forms known as 'cone-in-cone.' No fossils were found in them, but two thin seams of anthracitic coaly matter are stated to have been seen on their line of strike eastward of Courtney Bay.[13]

Overlying these beds, is a group of very different character. It consists of purplish-red and green grit and shale, with beds of red conglomerate and red sandstone. Interstratified with these are massive beds of a greenish rock, consisting of trappean and feldspathic fragments, imbedded in a shining reddish paste, or sometimes presenting the appearance of a compact trap or amygdaloid. This rock usually presents an appearance of greater alteration than the neighboring beds, and contains veins of epidote, quartz, and calc-spar. Its hard and massive character causes it to resist denudation, and to project above the surface in irregular masses. It has usually been regarded as a trap; I am disposed however, to consider it as more probably a tufaceous or volcanic ash rock, except in a few places, where it is either an amygdaloid trap or a mass of fragments of such material too

[13] Gesner's Second Report.

intimately connected to be separated from each other. It is evidently a stratified member of the series, though its beds are very unequal in hardness and texture, and probably also in thickness. This portion of the series is well exposed on the east side of Courtney Bay, in the southern part of the city of St. John, and in the direction of Carlton, where its tufaceous or trappean members constitute prominent elevations. It seems also to be this member of the series which, turning to the south, constitutes Cape Meogenes.

Reposing on the rocks last described, is the most interesting member of the series, consisting of hard buff and gray sandstones, with black and dark-gray shales. The sandstones contain numerous Coniferous trunks; and the shales, which are sometimes highly graphitic, abound in delicate vegetable remains, often in a very perfect state of preservation. These rocks appear on the east side of Courtney Bay, near Little River, at the extremity of the point of land on which the city of St. John stands, and in the ledges and cliffs on the shore westward of Carlton. In all these places they are quite conformable with the underlying rocks, though the dip gradually diminishes in ascending.

No rocks newer than the above are seen at Carlton or in the city of St. John; but near Little River a few beds of red shale and coarse sandstone seem to indicate the commencement of a new member of the series, the coast-section failing at this point. Mr. Matthew has, however, succeeded in finding a continuation of the section further inland, exhibiting first, in ascending order, gray sandstone and grit, with dark shale holding fossil plants, among which is *Calamites Transitionis*. This may perhaps be regarded as the top of the group last mentioned. Above it, and passing into it at their base, are reddish sandstones, grits, and conglomerates, alternating with green, greenish-gray, and red shale. Resting on these, is a thick-bedded, coarse, angular conglomerate, succeeded by evenly bedded shales, shaly sandstones. and grits, of dark-red and purplish colors. These are the highest beds seen, as beyond this place they are bent in a synclinal, and reappear with reversed dips.

Another most important observation of Mr. Matthew is that near Red Head the member of the St. John series last described is overlain unconformably by a conglomerate similar to that of the Kennebeckasis, and probably the Lower Carboniferous conglomerate. It dips to the northwest, or in the opposite direction from that of the underlying beds, at an angle of 30°; but Mr. Matthew regards the dip as due in part to false bedding.

The whole of the deposits above described may be summed up as follows, the thicknesses stated being from measurements

and estimates made by Mr. Matthew, and to be regarded as merely approximate.[13]

Carboniferous System.

Feet.

Coarse red conglomerate, with pebbles of the underlying rocks, and constituting in this vicinity the base of the Carboniferous System.

Devonian System (or perhaps, in part, Upper Silurian).

1. Dark-red and greenish shales; flaggy sandstones and grits; coarse angular conglomerate, - - - - - - - - 1850
2. Reddish conglomerate, with quartz pebbles; reddish, purple, and grey sandstones and grits; deep-red, gray, and pale-green shales. A few fossil plants, - - - - - - - - - 2350
3. Blackish and gray hard shale and arenaceous shale; buff and gray sandstone and flags. Many fossil plants; Crustaceans and *Spirorbis*, - - - - - - - - - - - - 2000
4. Reddish conglomerate, with slaty paste and rounded pebbles; trappean or tufaceous rock; red, purplish, and green sandstones and shales. Thickness variable, - - - - - - - - 1000
5. Black papyraceous shale, with layers of cone-in-cone concretions, - - - - - - - - - - - - - - - 400
6. Hard, generally coarse and micaceous, gray shales and flags, of various shades of color, and with some reddish shale and tufaceous or trappean matter at the bottom. *Lingulæ*, burrows, and trails of animals, - - - - - - 3000 feet or more.
7. White and gray crystalline limestone, with bands of shale and beds of graphite, - - - - - - - - 600 feet or more.
8. Gneissose and other metamorphic beds, with bands of quartz-rock and slate. Thickness unknown.

The Devonian age of the upper members of this great series of beds I regard as established by their fossils,[14] taken in connection with the unconformable superposition of the Lower Carboniferous conglomerate. The age of the lower members is less certain. They may either represent the Middle and Lower Devonian, or may be in part of Silurian age. Their only determinable fossil, the *Lingula* of the St. John shales, affords no decisive

[13] In my paper in the *Canadian Naturalist*, I gave a sectional view of the general arrangement, as observed on a line of section from the Kennebeckasis River to the extremity of the peninsula on which St. John stands. The sections referred to in the text represent the same series, as seen on the east side of Courtney Bay, immediately to the east of St. John, with the continuation ascertained by Mr. Matthew towards the Mispec River.

[14] The scanty animal remains of the plant-beds No. 3 accord very well with the evidence of the fossil plants. They are a small Trilobite, apparently a *Phillipsia*, and three other Crustaceans, one of which is probably a *Stylonurus*, another a *Eurypterus*, and the third a Decapod not apparently referable to any described genus. These Crustaceans are now in the hands of Mr. Salter. (See his paper on these fossils, read before the Geological Society, May 21, 1862.) There is also a shell, apparently a *Loxonema*, and a *Spirorbis*.

solution of this question, and the evidence of mineral character is not to be relied on in the case of beds so remote from those regions in which the Devonian rocks of America have been most minutely studied.

In mineral character, Nos. 1 and 2 of the above sectional list might very well represent the Old Red Sandstone, or Catskill group, of the New York geologists. Nos. 3 and 4 might be regarded as the analogues of the Chemung and Portage groups. No. 5 would represent the Genesee Slate; No. 6 the remainder of the Hamilton group; No. 7 the Corniferous Limestone; and No. 8 might be regarded as a metamorphosed equivalent of the Oriskany and Schoharie Sandstones. The entire want of the rich marine fauna of these formations is, however, a serious objection to this parallelism. If, on the other hand, we employ as our scale of comparison the development of the Devonian system in Gaspé, Nos. 1 and 2 will correspond very well with the upper member of the Gaspé series, and No. 3 with the rich plant-bearing beds of the middle of that series; but no mineral equivalent of the St. John shales and limestones occurs at Gaspé, unless we seek for it in the Upper Silurian.

The rocks of the St. John group extend along the coast as far as the frontier of Maine, and there can scarcely be any doubt that the plant-bearing beds at Perry represent some portion of the St. John series, most probably Nos. 2 and 3 of our sectional list. At Perry, the plant-beds rest on a trappean bed, which may be the equivalent of our No. 4, a member of the series much more constant in its occurrence than would be anticipated from its composition. According to Prof. Hitchcock, this last bed at Perry, rests unconformably on shales containing a *Lingula*, apparently not identical with that of St. John, and also other fossils of distinct Upper Silurian forms. The analogy of Perry, therefore, as well as of Gaspé, would point to an Upper Silurian age for the lower members of the St. John series, though at St. John they appear to be conformable with the overlying beds. On the other hand, the unconformability at Perry renders it possible that the lower members of the St. John series may be wanting there; and to assign a Silurian date to the lower beds at St. John would imply the entire absence of the copious and characteristic Lower Devonian marine fauna observed at Gaspé and in Nova Scotia, as well as in Maine, though not in immediate connection with the Perry beds; while, if the whole series of St. John be Devonian, the absence of this fauna would be accounted for by the metamorphism of the lower beds.

In the present state of the evidence, it would be premature to decide this question, which may be settled either by the discovery of portions of the lower beds in a less altered state, or by tracing the St. John series into connection with the similar

deposits in Maine. In the meantime, therefore, we may be content to regard the upper members of the series as belonging to the later part of the Devonian Period, leaving the lower members to be regarded as Lower Devonian or possibly Upper Silurian.

The fossiliferous portion of the St. John series presents the richest local flora of the Devonian Period ever discovered. It far excels, in number of genera and species, the Lower Carboniferous flora as it exists in British America, and is comparable with that of the Middle Coal-measures, from which, however, it differs very remarkably in the relative development of different genera, as well as in the species representing those genera.

It is only just to observe, that the completeness of the following list is due to the industrious labors of an association of young gentlemen of St. John, who, under the guidance of Messrs. Matthew and Hartt, have diligently explored every accessible spot within some distance of the city, and have liberally placed their collections at my disposal for the purposes of this paper.

Dadoxylon Ouangondianum *Dawson.*
Sigillaria palpebra, sp. nov.
Stigmaria ficoides (var.) *Brongn.*
Calamites Transitionis *Gœppert.*
—— cannæformis *Brongn.*
Asterophyllites acicularis, sp. nov.
—— latifolia, sp. nov.
—— scutigera, sp. nov.
—— longifolia *Brongn.*
—— parvula *Dawson.*
Annularia acuminata, sp. nov.
Sphenophyllum antiquum *Dawson.*
Pinnularia dispalans, sp. nov.
Lepidodendron Gaspianum *Dawson.*
Lycopodites Matthewi *Dawson.*
Psilophyton elegans, sp. nov.
—— glabrum, sp. nov.
Cordaites Robbii *Dawson.*
—— angustifolia *Dawson.*
Cyclopteris Jacksoni *Dawson.*
Cyclopteris obtusa *Gœppert.*
—— varia, sp. nov.
—— valida, sp. nov.
Neuropteris serrulata, sp. nov.
—— polymorpha, sp. nov.
Sphenopteris Hœninghausi *Brongn.*
—— marginata, sp. nov.
—— Harttii, sp. nov.
—— Hitchcockiana, sp. nov.
Hymenophyllites Gersdorffii *Gœppert.*
—— obtusilobus *Gœppert.*
—— curtilobus, sp. nov.
Pecopteris (Alethopteris) discrepans, sp. nov.
—— (——) ingens, sp. nov.
—— (——) obscura (?) *Lesquereux.*
Trichomanites, sp. nov.
Cardiocarpum cornutum, sp. nov.
—— obliquum, sp. nov.
Trigonocarpum racemosum, sp. nov.

(To be continued).

ART. XXXII.—*On the nature and advantages of the Globe Lens for the Photographic Camera;* by COLEMAN SELLERS.

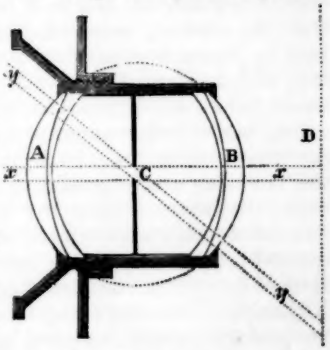
THE Globe Lens for photographic cameras, patented by Messrs. Harrison and Schnitzer of New York, is now attracting so much attention and is the subject of such contradictory statements, that a brief notice of it by one who has tried it may not be uninteresting to the readers of this Journal. Photography, with the discovery of the use of collodion, seemed to leap into its present high position at one bound, at least so far as the chemistry of

the art is concerned. The negatives of to-day look like the negatives of the first experimenters, and the chemical process of their production is essentially the same. But with the optics of photography the case is different—here there has been a steady improvement. The wants of the portraitists have been met by the construction of new objectives suited to the style of pictures to be produced. In these instruments depth of field with free admission of a large volume of light was what was most sought for. Theory could not dictate what shape or combination of lenses would best produce this result, and patient experiments were resorted to. The requirements of landscape photography are quite different from those of portraiture. A portrait tube may be used to take views if it be provided with a stop or small opening to limit the amount of rays passing through it and thus to deepen the field, or increase the 'reach' of the instrument as it is technically called. This involves loss of light, and consequently diminishes the quickness of its working. We hear continually of rapid or instantaneous photography, and are often led to believe that the rapidity is to be ascribed to some wonderful sensibility of the chemicals used, but this is only partially true, and to the optician is due the most of the merit of instantaneous pictures. A portrait tube with its full opening will, in a sky-light room, produce a picture in perhaps ten or fifteen seconds. This same instrument, with the same opening and same chemicals, exposed to an extended view in bright sun light, could not be opened and shut quick enough; the immense volume of light reflected from so large an area of space being concentrated on the same sized plate as in the first case, would be too violent in its action, and from the nature of the instrument near and distant objects could not be brought into focus at the same time.

The human eye, when the head is at rest, takes in an angle of view of at least 70° or 80°, the whole of which is not seen clearly at once but can be examined in detail by the almost unconscious rolling motion of the eye in its socket—the actual included angle of clear vision at any one instant being only 1° or 2°. Hence a picture of a landscape, for instance, to fill the eye and seem a true representation of nature, should include an angle of at least 60°. Ordinary instruments, such as have heretofore been used, do not include an angle of more than one-half this amount and hence has originated the complaint that photographic views represent mere patches of scenery and not pictures. I remember once standing on a bridge—camera in hand—and looking up the romantic Wissahicon. The picture presented to my eye was very beautiful—the centre a waterfall framed in on both sides by wild and rugged rocks and spanned above by the arch of a railroad bridge crossing at the tops of the cliffs. The foreground was made up of a stony bed, where danced and foamed the rapid cur-

rent. I planted the camera and hoped soon to peel off from this charming view a cuticle (as Dr. Holmes says) which like plates of mica could be split and re-split for the collections of my friends. But on the ground glass I found nought but the tumbling water. No rocks, no bridge, no stony river bed—the poor camera in its empty head was incapable of taking in the whole of the charming picture. One of the dreams of the photographer has been of an instrument which should embrace a large angle and thus satisfy the wants of the eye; but, with the majority of the attempts in this direction came other evils, the greatest of which was distortion of the marginal lines. The aplanatic lens of Grubble is said to comprise an angle of 70°, but in a view before me of Trinity College, Dublin, taken with this lens, there is a curvature of the straight lines of the roof of more than one-eighth of an inch in its length. Mr. Sutton's panoramic lens, a sphere of glass filled with water, includes a very large angle, over 100°, on the base line, but the pictures are produced on curved plates, which require curved holders, baths, and printing frames, and, in the case of architectural pictures, the right lines are distorted, unless the picture be bent to the curvature of the plate upon which it was taken, and thus viewed near the centre of the curvature.

The Harrison and Schnitzer globe lens consists of two achromatic meniscus lenses placed with their concave sides together, and so made that their outer curved surfaces form part of a perfect sphere and the light is admitted through an aperture placed midway between the two lenses, i. e., in the exact centre of the external sphere. The annexed figure represents one of these instruments, A and B being the meniscus lenses, and C the centre opening through which the rays of light pass. The focus of such a lens one and three-quarter inches in diameter is two and one-half inches for distant objects, measuring from the surface of the back lens to the ground glass D. The circle of light produced is five inches in diameter, and from this may be cut the ordinary 3″ square of a stereoscopic picture. The included angle of light in the five inch circle is 75°, and in a three inch square picture cut from it is contained just four times the area of any instrument I have ever tried, suited to similar work. The remarkable property of this lens consists in its absolute correctness of reproduction. If it is used for copying purposes, the marginal lines are copied as straight as the originals, and, if



a copy be made the same size as the original, the photographic copy will, if laid upon the original, match it in every line. I have said that the globe of $2\frac{1}{2}''$ focus will make a circle of light of $5''$ diameter. This is when a distant landscape is in focus. If it is used for copying, the circle of light increases in diameter as the object approaches the front lens and the ground glass recedes to focus, so that an instrument which will cover a given size plate for views will cover one of twice the size, when reproducing the size of the original. As the lenses increase in size and length of focus, the plates covered increase in size, and the amount of glass in the lenses bear a larger proportion to the brass work in which they are mounted, and hence the included angle of vision is increased, so that while in the $2\frac{1}{2}$ inch globe the included angle is 75°, in a 12 inch globe (that is, one of $12''$ focus) the included angle is over 90°. It has been said that the light, being admitted through a round hole in a plane plate in the centre of the instrument, must be much more intense at the centre of the field than at the margin, and some writers have stated this fault to be one of great magnitude. Practice however does not show the evil to be so great as they represent, if it exists at all. By reference to the cut, it will be seen, that the dotted lines $x\ x$ representing a pencil of light of the diameter of the centre opening passing through the axis of the instrument, and $y\ y$ representing a pencil of light passing through the same opening obliquely, doubtless the area of the centre one will be the largest, but as it passes through much thicker glass than the rays $y\ y$, may not the ultimate effect of each be equalized? I do not pretend to any knowledge of the theory of optics, and must confine myself to facts. In the trial of many of these lenses, of different sizes, I have never found the evil to exist, and all the pictures I have made with the globe lens are remarkable for the even illumination of the field. At the last two meetings of the Photographic Society of Philadelphia, (February, 1863,) the merits of these lenses have been discussed—full credit for correctness of reproduction was accorded to them by all; but the quickness of working was questioned by one gentleman, who stated that in broad sunlight he had exposed an engraving for several minutes and had obtained an under-exposed negative, while all others present who had tried them were unhesitating in their assertions that they were remarkably quick workers when the size of aperture was taken into consideration. A few days ago I placed in bright sunlight an engraving from the London Art Journal, and copied it on a $6\frac{1}{2}\times8\frac{1}{2}$ plate with the same size instrument as was used by the gentleman who questioned its quickness. An exposure of 25 seconds gave an impression which flashed up instantly under the developer, assuming great intensity in the light and showing unmistakable symptoms of over exposure, so that I can see no

reason why the same law should not hold good with these lenses as with others, viz.: that, with the same area of opening to admit light, the shorter the focus the quicker they will work. For interiors, the short focus and large angle of vision possessed by these instruments render them invaluable, and as they are provided with a revolving diaphragm plate in the centre (not shown in the cut) various sizes of aperture can be brought into position, just as the stops under the table of some compound microscopes are arranged, and thus the largest amount of light, consistent with sharpness, admitted.

In the English journals, when the accounts of this instrument were first published, it was denounced in no measured terms, as being constructed on erroneous principles, and the assertion has even been made that its very shape must give fearful distortion to marginal lines, but since it has been proved to be no failure, and its success is no longer an experiment, comes the unwilling acknowledgment: "The principle of its construction must insure correct marginal lines," and last of all comes the declaration that it is "old, very old." Everybody had been making them for years and there is no merit of invention due to the patentees! Granting that lenses may have been made with an external spherical focus, as is the Sutton case, it will be difficult to produce a lens, made previous to the invention of this now described, composed of two achromatic meniscus lenses combined as these are and producing a like result. The theory of operation and mode of construction of the globe lenses admit of their being readily made of various focal lengths, and thus, by the use of a series of instruments, the whole included angle can be made available on any size plate that may be desired; the six inch focus covering a $6\frac{1}{2}'' \times 8\frac{1}{2}''$ plate and the 12″ focus lens covering $14'' \times 18''$ each including the same angle. One great advantage of short-focus lenses, when there is no spherical distortion, is in the appearance of perspective produced. If, for instance, we would view a machine or statue to the best advantage, we stand at such a distance from it as will admit of our viewing the whole of it in the eye at once, and can then best judge of its proportion. If now a picture be made by an instrument of long focus, it will be so far away from the object to be depicted as to make, as it were, too nearly a plane or flat view of it, deficient in perspective effect. With the very shortest focus of this new lens (the $2\frac{1}{2}''$ focus), this perspective effect may be too much exaggerated, but with all the other sizes it is not, and with the globe lens and with this only have been produced pictures which seem to me to convey a just idea of size and proportion. Some year or so ago, Messrs. E. and H. T. Anthony published a series of stereographs of Niagara, which seemed to me when I first saw them to bring to my mind all the wonders of the stupendous

cataract, and all to whom I have shown them seemed similarly impressed: it was not until recently that I learned that they were taken with the Harrison globe lens, thus furnishing another convincing argument in my mind of the value of the instrument. I cannot too strongly urge their adoption by photographers, and am proud of them as originating in America. The shortness of their focus adds much to their portability, as the camera is made smaller than usual, and amateur field photography with the globe lens and dry plates is a pleasure in place of a labor. Its advantages may be summed up in a few words. Short focus, clear definition, wide angle of included vision, absolute correctness of copy on a plane surface, and tolerably quick work. It takes the place entirely of the orthoscopic lens, giving absolute correctness to marginal lines, while the orthoscopic was only approximately correct. It fills all the requirements of a lens for landscape and architectural work, and is wanting only in the one thing of absolute instantaneity of action.

Philadelphia, March 10, 1863.

[We may add to Mr. Seller's notice of the 'Globe Lens' that this instrument has been found to reproduce military and other maps and plans with a minute fidelity heretofore unattained; and by their use our Army and Navy are furnished with photographic copies of manuscript maps and Coast-Survey plans, in which, as appears from the statements of the officers in charge, there is no sensible distortion of the right lines, even on very large plates.—Eds.]

Art. XXXIII.—*On the Glacial origin of certain Lakes in Switzerland, the Black Forest, Great Britain, Sweden, North America, and elsewhere;* by A. C. Ramsay, F.R.S., President of the Geological Society, &c. (Communicated by the Author.)[1]

Erroneous theories of the transport of Alpine Blocks.—In the year 1859, in a series of papers by the members of the Alpine Club, I published a memoir in which I compared the old glaciers of North Wales with those of Switzerland; and in it, among other matters, I explained the glacial origin of certain rock-basins now holding lakes, on the watersheds and in the old glacier-valleys of both those countries; and in a later edition of the same memoir, published as a separate book, with additions,[2] I extended these generalizations to many of the lakes in Sutherlandshire.

In the same work I also expressed an opinion that the blocks

[1] From the Quarterly Journal of the Geological Society for August, 1862.
[2] 'The Old Glaciers of North Wales.' Longman & Co.

of Monthey, in the valley of the Rhone, and the great erratic boulders that strew the southern flank of the Jura had been transported by icebergs derived from glaciers which descended in the Alpine valleys to the sea-level, during a period of submergence in which the low country that lies between the Jura and the Oberland was covered with erratic drift.

There was nothing new in this latter opinion, for it had previously been held by several distinguished geologists, both English and continental.

Since then I have twice revisited Switzerland, and have seen good reason to change my opinion respecting the cause of the transport of erratic blocks to Monthey and the Jura, and of *débris* not remodelled by rivers, &c., that lies scattered over the lowlands of Switzerland, or that borders, or lies in great mounds well out, in the plain of Piedmont and Lombardy. I am now convinced, for example, that the vast circling moraine of Ivrea, noticed by Studer in 1844, was shed from a glacier, 105 miles in length, that filled the valley of Aosta to a height of more than 2000 feet, and protruded far into the plain; while on the north a still greater glacier, long ago described by Charpentier, flowed from the valley of the Rhone right across the low country until its end abutted on the Jura. As there are still many persons in England who doubt these conclusions, it may not be beside the question to state the considerations that led me to reject the old theory.

Reasons for abandoning the older theories.—I first began to doubt the correctness of my earlier opinions in the summer of 1860, while examining the country near Bonn, the banks of the Moselle, and the Eifel. Neither in the valleys nor on the wide table-lands on both sides of the Rhine and the Moselle is there any sign of glacial drift. Excepting alluvial *débris* in the valleys, the native rock is generally quite bare of transported detritus; and the only marks of glaciation lie low on the sides of the Moselle, where the floating down of the river-ice has frequently rounded, polished, and striated the rocky banks in the direction of the flow. Boulders, transported from further up the stream, also sometimes lie on the shores. But, in the absence of true drift, I considered that, had Switzerland been depressed at least 3000 feet, until its mountains were washed by a sea that floated transported blocks to the higher Jura, the table-lands of Rhenish Prussia and Westphalia would also possibly have been submerged, and more or less covered with glacial detritus. Further up the Rhine and in the Black Forest the same absence of marine drift prevails. There, looking eastward towards the Rhine, the mountains, chiefly of gneiss, are wonderfully scarred, telling the observer of the wasting effects of

frost, ice, rain, and rivers, probably ever since the close of the Miocene period. In the valley of Oberweiler, between Mullheim and the watershed, I observed occasional heaps of moraine-like detritus, in which by diligent searching I found a few stones marked with the familiar glacial scratchings.

In the interior towards Schonau and the Belchen, the rocks being generally soft and schistose, no very decided signs of old glaciers occur, and no part of the country shows symptoms of the presence of drift. Altogether, the country looks as if it had stood in the air for so great a period that, even if glaciers were once present, they had disappeared so long ago that all the more prominent signs of degradation are now due to rain and running water. But further in the interior it is altogether different; for the signs of old glacier-ice are plentiful enough, and for miles round the Feldberg, which rises 4982 Baden feet above the sea, the sides of the valleys to the very summits of the mountains are often strikingly *moutonnées*, though the rounded forms are generally roughened and frequently half ruined with age. On these, striations, though rare, may occasionally be discovered (running in the direction of the valleys), although the rapid rate at which the rock weathers is much against their preservation. Moraines also are not uncommon. At the foot of the Feldberg, on the east, there is a beautiful circular lake, called the Feldsee, surrounded by tall cliffs of gneiss and granite in the shape known in Scotland as a corrie—a form eminently characteristic of all glacier-countries past or present. The outer side of the lake is dammed up by a perfectly symmetrical moraine, curving across the valley, and formed of sand, gravel, and of granite and gneiss, often in large boulders. It is now covered with pine-trees. The lake is deep, and the moraine rises from 25 to 40 feet above the water. Outside the moraine lies a flat marsh, still retaining traces of having been a lake, once also dammed by a second and outer moraine, formed chiefly of large angular blocks of gneiss, piled irregularly on each other like the old moraine of Cwm Bochlwyd, above Llyn Ogwen in Caernarvonshire. Quantities of moraine-matter strew the valley for two or three miles further down to the little marshy lake at Waldbauer, which is also dammed up by moraine-rubbish, in one place rudely stratified, like some of the old moraine-heaps on the Jura and parts of the great moraine of Ivrea; or like the heaps of glacier-*débris* that often border the lakes, marshes, and flat peat-mosses, once lakes, that diversify the lowlands of Switzerland. At the upper end of the Alb Thal also, at the entrance of Menzenschwanden Alb, I saw four moraines curving across the valley, arranged concentrically one within another, like those at the end of the glacier of the Rhone; and for many miles in the Alb Valley, both above and below St. Blasien,

roches moutonnées stand like islands through the alluvium, while it is also plain that the sides of the mountains above have been to a great height smoothed by ice. Nowhere, however, down to Allbruck, where the river joins the Rhine,[1] did I see any "drift;" and this village lying close on the north side of the Jura, it seemed impossible that the higher ground on the south side of that range, between the Lakes of Constance and Geneva, should have been submerged during any part of the Glacial period, while the country on the Rhine above Basel remained above the sea. I therefore saw that the theory that the *Pierre à bot* and its companion blocks had been floated from the Alps by marine icebergs was untenable; and a later examination of a portion of the Jura, partly under the able guidance of Professor Desor, fully convinced me that the ice that descended the great valley of the Rhone had covered much of the low country and abutted on the south-eastern flank of the Jura.

Old distribution of the Great Alpine Glaciers.—At that period, then, of extreme cold, when the glaciers of the Alps flowed right across the Miocene basin of Switzerland, a glacier of vast thickness, running from end to end of the upper valley of the Rhone, debouched upon the lowlands at what is now the eastern end of the Lake of Geneva, and, spreading in a great fan-shaped mass, extended to the south-west several miles down the Rhone below its present outflow from the lake, and north-east to the banks of the Aar, about half-way between Solothurn and Aarau. The length of this fan-shaped end of the glacier, from north-east to southwest, was about 130 miles, and its extreme breadth about 25 miles. Another great glacier descended in a direction opposite to the higher part of the Rhone glacier, through the upper valleys of the Rhine, and debouched upon a wide area that extends from Kaiserstuhl on the Rhine, far to the north-east. In the center of this area lies the lake of Constance. Between these, which were the largest glaciers on the north watershed of the Swiss Alps, several smaller, but still enormous, glaciers flowed in a north-westerly direction from the mountains, —one down the Linth, through the area now occupied by the Lake of Zurich, another down the Upper Reuss, across the area in which lie the Lakes of Lucerne, Zug, and others, and a third down the valley of the Aar to Berne, through the country that now contains the Lakes of Brienz and Thun. According to this view (the result of the researches of the best Swiss geologists), the greater part of the Swiss Miocene area lay deep under ice, and I am inclined to think that the country between the great old glaciers of the Reuss, Aar, and Rhone was much more covered with ice than any map shows, the whole helping to

[1] Between Basel and the confluence of the Aar and the Rhine.

swell the prodigious glacier of the Rhone that abutted on the Jura.

Connection between Tarns and Glaciers.—In *The Old Glaciers of North Wales* I have shown that in all glacier-countries, whether whether past or present, there is an intimate connexion between tarns and glaciers. Some of these are dammed by old moraines,[a] but the greater number lie in *rock-basins*, formed by the grinding of glacier-ice as it passed across the country, whether in valleys, on rough table-lands, or on the watersheds of passes. These lakes and pools are of all sizes, from a few yards in width, lying amid the mammillations of the *roches moutonnées*, to several miles in diameter. Sometimes in the convolutions of the strata (conjoined with preglacial denudation subsequent to the contortion of the beds), softer parts of the country may have been scooped out, leaving a hollow surrounded by a frame-work of harder rock; but perhaps more generally they were formed by the greater thickness and weight, and consequently proportionally greater grinding pressure, of glacier-ice on particular areas, due to accidents to which it is now often difficult or impossible to find the clue. Trifling as this phenomenon at first sight may seem, I yet believe the manner of the formation of these lakes is of much importance to the right understanding of the glacial theory, whether taken in connection with the great extension of extinct glaciers in recognized glacier-regions, or, further, when viewed on a general continental scale; for *the theory of the glacial origin of many rock-basins* must, I feel convinced, be extended much beyond such mountain-districts as Switzerland, Wales, and the Highlands of Scotland, where they first attracted my attention.[b]

Origin of the Great Alpine Lakes, subject stated.—From the consideration of the origin of mountain-lakes and tarns, the question easily arises,—What are the causes that have operated in the formation of the great lakes of Switzerland, such as those of Geneva, Zurich, and Constance, and, south of the Alps, of Maggiore, Lugano, Como, and others? To answer this with precision, it will be necessary, first, to examine several other hypotheses that by some may be thought sufficient to account for them.

It is well known that after the close of the Miocene epoch the rocks of the Alps were much disturbed,—a circumstance

[a] Quart. Journ. Geol. Soc. Lond. 1851, viii, 371; and The Old Glaciers of North Wales.

[b] It is not to be supposed that I attribute the origin of all rock-basins to glacial action. Many lie in the craters of extinct volcanos, some, no doubt, in areas of special subsidence, and others may be due to causes of which I know nothing. I now confine my remarks to certain lakes common in all highly glaciated regions such as I know.

proved by the contortion of the Miocene strata, as for instance in the neighborhood of Lucerne, where, on the Rigi (and in other conglomeratic mountains on the same strike), the strata are considered by the best Swiss geologists to be repeatedly folded and fairly inverted, so that the basement-beds form the top of the mountain, instead of its bottom, thus, by reversal of dip, plunging under the Eocene and Cretaceous strata of the mountains further south. The whole, as shown by the rapid truncated folding and the escarpments of the hills, has since been much denuded, the denudation being of a kind and amount that, to effect it, proves the lapse of a long period of time. Witness the outliers of Miocene strata in the upland valleys of the Jura. Among these disturbed and denuded strata of Miocene and of older dates, the Lakes of Geneva, Thun, Brienz, Lucerne, Zurich, Constance, the Wallen See, and the great lakes of north Italy lie. A knowledge of the stratigraphical structure of the Alps, in my opinion, proves that these lakes do not lie among the strata in basins merely produced by disturbance of the rocks, but in hollows due to denuding agencies that operated long after the complicated foldings of the Miocene and other strata were produced.

First, none of these lakes lie in simple synclinal troughs. It is the rarest thing in nature to find an anticlinal or a synclinal curve from which some of the upper strata have not been removed by denudation. I never yet saw a synclinal curve of which it can be proved that the uppermost stratum in the basin is the highest layer of the formation that was originally deposited over the area before the curving and denudation of the country took place. The only approach to this may possibly be in the upper valleys of the Jura, where a part of the Miocene beds lie in basins separated by secondary anticlinally curved strata, the tops of the anticlinal bends having been removed by denudation; but these cases are surrounded with difficulties. The lake-hollows in the Alps are, however, encircled by rocks, the strikes, dips, and contortions of which often exhibit denudation on an immense scale; and in no case is it possible to affirm, here we have a synclinal hollow of which the original uppermost beds remain. If these beds have disappeared to a great extent, then it is evident that denudation has followed disturbance. The fragmentary state of the uppermost Miocene strata of the lowlands of Switzerland proves this denudation. Again, if it be argued that in the lake-areas these denudations have been produced by the waters of the lakes, it is replied that, though waves may form cliffs, neither running nor still water can scoop out deep trough-shaped hollows.

Secondly, the same kind of argument applies to areas of mere watery erosion by rivers. Running water may scoop out a

sloping valley or gorge, but (excepting little swallow-holes) it cannot form and deepen a profound hollow, so as to leave a rocky barrier all round: though it may fill with sediment one that had previously been formed.

Thirdly, neither do most of the Swiss lakes lie in lines of dislocation. For many reasons, I do not believe that any one of them among the high Alps or on their flanks can be proved to lie in lines of mere gaping fracture. Let us consider the nature of such fractures.

In any country where the strata are comparatively little disturbed and lie nearly horizontally, if it be faulted, there is no reason why the fractures should be open. In the Oolites, for example, in the south of England, where faults are numerous, and in the New Red Sandstone of the central counties, there is generally a simple displacement of the strata up or down, on one side or the other; or, if the disturbance go beyond this, it is that along the sloping line of fracture the beds on the downthrow side are turned up, and those on the opposite side bent down, by pressure and slipping combined. In more disturbed districts, like the Welsh Coal-measures, the same phenomena are observable: witness, for instance, the numerous sections from accurate observation, drawn on a true scale, by Sir Henry De la Beche, Sir William Logan, and others. Experience, both above ground and in mines, proves the same. Most lodes are in fractures, and many lie in lines of fault. In metamorphic, excessively contorted, and greatly fractured districts, like those of Devon, Cornwall, and Wales, the cracks, whether bearing metals or not, vary from mere threads to a few fathoms in width. They are always filled with quartz or other foreign substances, frequently harder than the surrounding matrix. I have often traced lodes on the surface, in Wales, by the hard matter filling the crack standing in relief above the surface of the softer enclosing rock. In limestone rocks the cracks are usually partly filled with crystallized carbonate of lime. Lines of fracture are not, therefore, for purposes of denudation, necessarily lines of weakness, unless it happen that on opposite sides of the fault hard and soft rocks come together, when of course the softer rocks will wear away more rapidly, and generally originate a straight valley.

Again, in an excessively contorted country, such as the Alps, it is, I believe, impossible, *in consequence of that contortion*, that there should be gaping fractures now exposed to view. Assuming for the sake of argument the sudden violent contortion of the strata of any great tract of country, we shall see that the contorted rocks *now exposed at the surface*, even if broken, would be most unlikely to gape.

The expression "elevation of mountains" conveys to the minds

of many persons the idea that the elevation has been produced by some force acting from below, along a line in the case of a chain, and on a point of greater or less extent when the mountains lie in a cluster, as a whole, more or less dome-shaped. Such forces would stretch the strata; and, when they could no longer stand the tension, cracks would ensue, and many lines of valley are assumed to lie in such fractures. But in Wales, the Highlands of Scotland, and more notably in the Alps, the strata now visible have been compressed and crumpled, not stretched, and they occupy a smaller horizontal space than they did previous to the formation of the chain.

Let us suppose a set of strata of (say) 14,000 to 20,000 feet in thickness, like the rocks of North Wales, and let these be spread out horizontally over thousands of square miles. Let these strata, from any cause, be compressed from the right and left so as to be contorted, and occupy a smaller horizontal area than they did before disturbance. Then, at a great depth, where the superincumbent strata pressed heavily on the lower beds, the latter would be crumpled up, cleavage would often supervene, and gaping fractures would be impossible; for, where mere fractures occurred, the walls of the cracks would be pressed more closely together. But nearer the surface, where there was less weight, and at it, where there was none, the beds would extend into larger curves than they did lower down; and where the limits of extensibility were passed, shattering might take place, and yawning chasms might ensue. In all violently contorted countries, however, as in the cleaved rocks of North Wales, for instance, the present surface shows those originally deep-seated contortions that since disturbance have been exposed by denudation; otherwise the rocks would not be cleaved. I therefore do not believe that in any country I have seen, such as Wales or Switzerland, there are any lakes now occupying yawning fractures, consequent in Switzerland on Post-eocene or Post-miocene disturbances. On the contrary, they lie in hollows of denudation, shortly to be explained, of later date than these disturbances.

Fourthly, again, it may be supposed that the great lakes lie each in an area of special subsidence; but, in reply to this, it is evident that among the unnumbered lakes of Switzerland and Italian Alps it would be easy to show a gradation in size, from the smallest tarn that lies in a rock-basin to the Lakes of Geneva and Constance. Neither do I see any reason why mere size should be considered the test of subsidence. Disallowing that test, we should require a great number of special subsidences, each in the form of a rock-basin, in contiguous areas. Between the Seidelhorn and Thun, for example, we should require one for the Todten See, several on the plateau on the north imme-

diately under the Seidelhorn, one for the lake at the Grimsel, another for the drained lake at the Kirchet,[6] and another for the lakes of Brienz and Thun. In Sutherlandshire these areas of special subsidence would be required by the hundred, and in North America by the thousand.

Signor Gastaldi, in a masterly memoir on the composition of the Miocene conglomerates of Piedmont,[7] considers with reason that the large angular blocks of these strata, many of them far-transported, and some of them foreign to the Alps and Apennines, have been deposited from ice-rafts; and thence he infers the existence of glaciers during a part of the Miocene epoch. But, admitting this, it is evident that the distribution of the Post-pliocene glaciers of the Alps must, in all details, have been quite different from those of Miocene age, in consequence of the great disturbance that the Alpine rocks underwent after the close of the Miocene epoch, and the subsequent formation of numerous new valleys of denudation. Traces of the long lapse of time between the Miocene and the later Glacial epoch are in other countries but imperfectly preserved in the subdivisions of the Crag, and of other minor formations of still later date. Of the finer gradations that unite these subdivisions, few traces have been described. For long before, and during all these Crag epochs and the ages between them, of which we have little trace, and during all the time that elapsed from the close of the Crag until the period of extreme cold came into action, the Alps stood above the sea, and suffering subaerial denudation, valleys were being formed and deepened. It is possible that, while the mild climates of the Lower Crag epochs endured, there may still have been glaciers in the higher Alps; but at whatever period the later glaciers commenced, those who allow the extreme slowness of geological change will admit that the period was immense that elapsed during the gradual increase of the glaciers, until, in an epoch of intensest cold, the ice abutted on the Jura in one direction, in another spread far beyond the present area of the Lake of Constance, and on the south invaded the plains of Lombardy and Piedmont. During all that time, weather and running water were at work modifying the form of the ground under review. But, as I have already explained, these two agents were incapable of scooping out deep hollows surrounded on *all* sides by rocks, and it therefore follows that the lakes first appeared after the decline of the glaciers left the surface of the country exposed approximately as we now see it,—unless we admit, what seems to me impossible, that fractures, formed at the close of the Miocene epoch, remained filled with water until

[6] See the "Old Glaciers of Switzerland and North Wales."

[7] "Sugli elementi che compongono i conglomerati Mioceni del Piemonte," Turin, 1861.

the great glaciers filled them with ice; or believe, with De Mortillet, that the valleys and lake-hollows were charged with water-borne alluvial or diluvial *débris* before the glaciers ploughed it out.[8]

Allowing the hypothesis of De Mortillet, the rock-basins must have been twice filled with water; but, according to my hypothesis, they did not exist as lakes till after the disappearance of the glaciers.

But the glacier map of ancient Switzerland shows that the areas now occupied by the great lakes, both north and south of the Alps, have all been covered with glaciers. No Tertiary deposit, of an age between the close of the Miocene and the commencement of the Glacial epoch, lies between the Alps and the Jura; and, had the hollows of the lakes existed prior to the great Glacial epoch, we ought, but for some powerful wasting agent, probably in these hollows, still to find some traces of fresh water deposits, perhaps of the age of part of the Crag. No such relics exist.

The Great Lakes. Lake of Geneva.—The Lake of Geneva is about 45 miles in length by about 12 in breadth, and its delta, once part of the lake, between Villeneuve and Bex, is 12 miles long. The latter and a small part of the banks of the lake beyond the mouth of the river lie in the great Rhone valley, formed of older Tertiary and Secondary rocks. All the rest of the lake is surrounded by the low country formed of the various subdivisions of the Molasse and Nagelfluh. The lake is 1230 feet above the level of the sea, and 984 feet deep towards the eastern end, according to the sounding of De la Beche.[9]

Geneva itself stands on superficial *débris;* but the solid rock first appears in the river-bed below Geneva, at Vernier, at the level of 1197 feet above the sea—only 33 feet below the surface of the lake, or 951 feet above the deepest part of its bottom. Any one acquainted with the remainder of the physical geography of the country will therefore see that the water of the lake lies in a true rock-basin. The question thus arises, How was this basin formed?

1st. It does not lie in a simple synclinal basin; for, though the Lake of Geneva lies in the great synclinal hollow of the Miocene strata between the Alps and the Jura, it is evident by an inspection of the country that the flexures of that formation are of far greater antiquity than the lake. These flexures have

[8] See an admirable memoir by G. de Mortillet, "Des Anciens Glaciers du Versant Italien des Alpes." Milan, 1860. Though I had seen his map, I had not seen this memoir when I read my paper; and the passages in which it is mentioned have been added as these pages pass through the press. His theory leaves the difficulty of the first formations of the basins untouched, unless we believe (which I do not) that the Alpine valleys are lines of fracture.

[9] Edinburgh Philosophical Journal, 1820, ii, 107, and plate 2.

been denuded, and the lake runs in a great degree across their strike.

2nd. For reasons already stated, it is, I believe, impossible to prove that the lake lies in an area of special subsidence, all the probabilities being against this hypothesis.

3rd. It is almost needless to say that the Lake of Geneva is too wide to lie in a mere line of fracture; and I know of no reason why the valley of the Rhone, where occupied by the delta, should be esteemed a line of fault or gaping fissure, any more than many other valleys in Switzerland, which many geologists will consider with me chiefly the result of the old and long-continued subaerial denudation of highly disturbed strata. I could enter on details to prove this point, but they belong rather to the rock-geology of Switzerland than to the matter in hand.

4th. Those who do not believe in the existence and excavating power of great and sudden cataclysmal floods will at once see that the area of the lake cannot be one of mere watery erosion; for not ordinary running water, and far less the still water of a deep lake, can scoop out a hollow nearly 1000 feet in depth.

Now, if the lake of Geneva do not lie in a synclinal trough, in an area of subsidence, in a line of fracture, or in an area of mere aqueous erosion, we have only one other great moulding agency left by which to modify the form of the ground, namely, that of ice.

When at its largest, the great glacier of the Rhone debouched upon the Miocene beds where the eastern end of the Lake of Geneva now lies. The boulders on the Jura, near Neuchâtel, prove that this glacier was about 2200 feet thick where it abutted on the mountains; and, where it first flowed out upon the plain at the mouth of the valley of the Rhone, the ice, according to Charpentier, must have been at least 2780 feet thick.[10] Add to this the depth of the lake, 984 feet, and the total thickness of the ice must have been about 3764 feet at what is now the eastern part of the lake. I conceive, then, that this enormous mass of ice, pushing first northwest and then partly west, scooped out the hollow of the Lake of Geneva most deeply in its eastern part opposite Lausanne, where the thickness and weight of ice, and consequently its grinding power, were greatest. This weight, decreasing as it flowed towards the west, from the natural diminution of the glacier, possessed a diminishing eroding power, so that less matter was planed out in that direction, and thus a long rock-basin was formed, into which the waters of the Rhone and other streams flowed when the climate ameliorated and the glacier retired.

[10] The Lake of Geneva is 197 feet lower than the Lake of Neuchâtel. The glacier first surmounted the hills between Lausanne and Vevay, and then flowed down the general slope northwards to the Jura.

Lake of Neuchâtel.—The basins of the lakes of Neuchâtel, Bienne, and Morat were, I consider, hollowed out in a similar manner, differing in points of detail. Near the Lake of Neuchâtel, on the flank of the Jura, the fan-shaped end of the Rhone glacier attained its greatest height, swelled in size and pressed on as it was by others that descended from the north snow-shed of the mountains between the Oldenhorn and the great snow-field above Grindelwald. According to estimates based on the highest ice-stranded boulders, the ice rose 2203 feet above the present surface of the lake. The lake is now 1427 feet above the sea, and 480 feet deep; and the Lake of Bienne is 1425 feet above the sea, and 231 feet in depth. The bottom of the Lake of Neuchâtel is thus 947 feet above the sea. Unless the gravel, therefore, on the banks of the Aar, immediately east of the latter, be over 480 feet deep, the hollow of the lake near its immediate bounds is a true rock-basin; for on the north, south, and west it is surrounded by solid Secondary and Miocene rocks. Even if the rock does not rise close to the surface in the river near the lake, still, at Solothurn, strata in place come close to the river-bank on both sides, the river being 1414 feet above the sea. Under any circumstances there must therefore be a long, deep trough between Solothurn and the rocks a little southwest of the Lake of Neuchâtel. How was this basin formed? When the glacier, debouching from the valley of the Rhone, spread out like a fan and pressed forward till it abutted on the Jura, its onward progress was stopped by that mountain; and direct further advance being hindered, the ice spread northeast and southwest, to the right and left, and being as a whole thickest and heaviest above the area where the lake now lies, a greater quantity of the Miocene strata on which it rested must have been ploughed out there than further on towards the northeast and southwest ends of the glacier, towards which the ice, gradually declining in thickness, exercised less grinding power. In this manner I believe the troughs were formed in which lie the three lakes near Neuchâtel; and when the ice finally retreated, the ordinary drainage of the country filled them with water, the cliffs on the southeastern side of the Lake of Neuchâtel and other changes of the form of the ground having since been produced or modified by watery erosion and the local deposition of silt and alluvial gravel.

The Lake of Thun.—The Lake of Thun is 1825 feet above the sea, and 776 feet deep. Its bottom is therefore 1049 feet above the sea. It is about 10 miles in length, 1½ broad, and its length chiefly cuts across the strike of rocks of Secondary and Miocene age. The Lake of Brienz (about the same size) is more remarkable; for, while its level is 1850 feet above the sea, its depth is more than 2000 feet; so that its bottom is at least between 100

and 200 feet below the level of the sea. Before the formation of the alluvial plain between, these two lakes were probably united; and whether or not this was the case, it is evident, from its great depth, that the Lake of Brienz lies in a true rock-basin. Even if below Thun the rocks do not crop nearer than Solothurn, the Lake of Thun still lies in a rocky hollow more than 600 feet deep, both hollows having, I believe, been deepened by the great old glacier of the Aar, the ice of which was so thick, that above Brienz it overflowed into the valley of Sarnen by the Brunig, about 1460 feet above the Aar below Meyringen, and sent off a branch which scooped out the hollows of the Lakes of Lungern and of Sarnen on its course towards Alpnach on the Lake of Lucerne.

The Lake of Zug.—The Lake of Zug is about 9 miles long, from 1 to 2½ wide, 1361 feet above the sea, and 1279 feet deep; and its bottom is therefore only 82 feet above the sea. The whole is surrounded by Miocene strata, the strike of which the lake cuts across, and its great depth clearly shows that it lies in a rock-basin.

The Lake of Lucerne.—The Lake of the Four Cantons (Lucerne) ramifies among the mountains and extends its arms in various directions. In its lower part, the branches that run N.E. to Kussnach and S.W. towards Gestad lie partly in the strike of the Miocene and older strata; but for the most part it runs across the average strike of the Eocene and Secondary rocks, between banks, sometimes precipitous, that rise in noble cliffs sometimes more than 2000 feet above the water. Its height is 1428 feet above the sea, and its recorded depth 853 feet; but the shape of the banks and the round number of 800 French feet make it likely that it may contain deeper gulfs than have yet been plumbed. If not, then its bottom is 575 feet above the sea; and those acquainted with the shape of the ground by Lucerne will easily be convinced that the lake lies in an actual rock-basin. The steepness of the walls of this lake more resembles the sides of a rent than those of any of the basins yet described, and the re-entering angles of rock opposite curving bays have been cited as evidences of fracture, one side being supposed to fit into the other. But in most cliffy valleys of aqueous erosion there are necessarily such re-entering angles, from the common action of running water; and, in Switzerland, ere these valleys were filled with ice, they existed in some shape, and were drained by rivers that deepened them and gave them a general form preparatory to the flow of the ice that largely modified their outlines. I should no more consider the re-entering angles a sign of gaping fracture in these valleys than I would the bends of the Welsh valleys or of the tortuous Moselle. But even if at first sight one were inclined to believe the space between the oppo-

site cliffs between Brunnen and Flühlen to be an open fracture, if we take a moderate average slope for each side, say of 65°, and produce it below the water, we get a depth, ere the lines meet, of between 7000 and 8000 feet—a very improbable depth for the original hollow of the lake. But it may be said that the fracture has been much widened by degradation, the line of the break merely giving a line of weakness, along which the surface-drainage might widen the valley. If, however, we only take an angle for the sides of the lake giving a moderate depth, the necessity for a fracture does not exist, and we recur to some process of mere erosion for the scooping of the hollow in which the water lies, that process having, I consider, been the long-continued grinding of the ice of the great glacier.

The Lake of Zurich.—The Lake of Zurich runs from N.W. to S.E., across the average strike of the Miocene strata, which are much disturbed towards its eastern end. It is bounded by high hills, much scarred by the weather, on which the different Miocene strata often stand out in successive horizontal steps. The Linth Canal and the Wallen See lie in an eastern prolongation of this valley, which is still further extended to the valley of the Upper Rhine at Sargans. The lake is about 25 English miles in length, by 2¼ wide in its broadest part. A great moraine partly dams it up at its outflow at Zurich; and a second forms the shallow at Rapperswyl, where the lake is crossed by a long wooden bridge. The general level of the water is 1341 feet above the sea, and only about 639 deep; and the bottom of the lake is therefore 702 feet above the sea. The limestone rocks at Baden, on the Limat, are 1226 feet above the sea; and the lake therefore lies in a true rock-basin, though it is probable that the old moraine at Zurich accounts for the retention of the water of the lake at its precise level. The long hollow was in old times entirely filled by the great glacier which descended from the mountains between the Todi and the Trinserhorn, through the valley of the Linth, to Baden.

The Wallen See.—The Wallen See lies in a deep valley, whose cliffy slopes of Secondary rocks rise from 2000 to 3000 feet, and in the Leistkamm 4500 feet above the surface of the lake. The lake itself is 1391 feet above the sea; and from the great steepness of its banks it may be inferred that it is exceedingly deep, but none of the authorities I have consulted give its soundings. A large branch from the great Rhine glacier joined that at the valley of Glarus and Zurich through this wide gorge, and ground out the hollow of the Wallen See.

The Lake of Constance.—The Lake of Constance, the largest sheet of water in Switzerland, is about 50 miles in length, by about 15 in breadth at its broadest part. It is entirely surrounded by Miocene strata, often considerably disturbed, and

forming great hills towards the S.E., which in a remarkable manner evince all the signs of long-continued erosion by running water, conveying the impression that chiefly by that means all the deep valleys of the district have been worn since the close of the Miocene epoch. This lake lies 1298 feet above the sea; and, its depth being 912 feet, its bottom is only 386 feet above the sea. The falls of the Rhine are 1247 feet above the sea; and the lake therefore lies in an unmistakable rock-basin, the whole of which was once overflowed by the deep and broad-spreading glacier of the Upper Rhine valleys which stretched far northward beyond the lake into Baden and Wurtemberg. Being of greatest thickness where it entered the region of the lake, by its enormous weight and grinding power it scooped out, in the soft rocks below, the wide hollow now filled with water.

The Italian Lakes.—If we now turn to the Italian side of the Alps, we shall find the same phenomena prevailing in the Lakes of Maggiore, Lugano, and Como, the only important lakes I have yet had an opportunity of seeing, south of the great chain. To each of these the same reasoning applies, modified only in detail; and I shall therefore briefly pass them over.

The most westerly, the Lago Maggiore, lies in a winding valley, 40 miles long, excavated in gneissic and Jurassic rocks, which rise on either side in lofty mountains. The surface of the lake is 685 feet above the level of the sea, and near the Borromean Islands it has the enormous depth of 2625 feet; so that its bottom is 1940 feet lower than the sea-level. It must, therefore, be enclosed all round by rocks, unless we suppose the narrow passage at Arona, near its outlet, to be as deep as its deepest part, or that the alluvial deposits of the Ticino and the Po are more than 1940 feet deep—an assumption no one is likely to make.

Of all the Alpine lakes, that of Lugano is the most irregular in form,—in the language of Mr. Desor, stretching its arms like a great polyp among the mountains in all directions.[11] Its surface is 938 feet above the level of the sea, and its depth 515 feet. Its bottom is therefore only 410 feet above the sea-level, and the shape of the surrounding ground renders it impossible to believe that it is not entirely surrounded by rocks.

The Lake of Como, the hollow of which has been scooped out generally in the same set of rocks as the other two lakes, is 700

[11] See memoirs "De la Physionomie des Lacs Suisses" (extrait de la 'Revue Suisse,' 1860) and "Quelques Considérations sur la Classification des Lacs, à propos des bassins du revers méridional des Alpes," by E. Desor. The opinions of Mr. Desor and my own do not agree on the question of the origin of the lake-basins on the Alps. His views are well expounded in the above-named memoirs. It was in conversation with my friend, in 1860, that I first proposed what I consider the true solution of the question, and to this conversation I presume he alludes in the latter memoir, p. 13.—"On a prétendu que les lacs étaient l'effet de l'affouillement des glaciers qui auraient labouré le sol sur lequel ils s'avancaient," &c.

feet above the sea, and 1929 feet deep; and its bottom is therefore 1229 feet below the level of the sea. On the borders of these lakes the rounded rocks and the well known glacier-stranded boulders, high on the mountain-sides, attest that these deep valleys were filled to the brim by a vast system of glaciers that flowed southerly from the snow-shed that runs from the eastern side of Monte Rosa, by the Rheinwald-horn, to the top of the valley of the Adda,—a system of glaciers so large that, like that of Aosta and Ivrea, further west, they protruded their ends and deposited their moraines far south on the plains of Piedmont and Lombardy.

The glacier of Ivrea, when it escaped from the valley of the Doire, deposited a moraine at its side, east of the town of Ivrea, rising in mere *débris* 1500 feet above the plain, and spreading out eastward in a succession of fan-shaped ridges miles in width. The vastness of this mass gives a fair idea of the huge size of the glacier, and of the great length of time it must have endured; and just as this glacier hollowed out the little rock-basins in which lie the tarns that nestle among the large *roches moutonnées* between the town and the moraine,[12] so, deep as the hollows of the great lakes of Maggiore and Como are, I believe they also were scooped out by the grinding power of long-enduring ice, where, under favorable circumstances, the glaciers were confined between the mountains, and therefore thicker than the glacier of Ivrea where it debouched on the plain. Diagrams illustrative of this subject should be drawn on a true scale; otherwise, height, depth, and steepness being exaggerated, the argument becomes vitiated. I have not the data for giving an actual outline of the bottom of the Lago Maggiore; but a line drawn from the upper end of the lake to the required depth near the Borromean Islands gives *an angle only of about* 3° *in a distance of about* 25 *miles*, and from thence to the lower end of the lake (12 or 13 miles) *of about* 5°. The depths of Maggiore and Como do not, in my opinion, militate against my view; for, if the theory be true, depth is a mere indicator of time and vertical pressure in a narrow space. It is interesting, and confirmatory of this view, that the deepest part of the Lago Maggiore is just at the point where the enormous glacier of the Val d'Ossola joined the great ice-stream that was formed by the united glacier-drainage of the valleys above Bellinzona and Locarno. Where these glaciers united, there the lake begins; and where the ice was on the largest scale near the Borromean Islands, there the lake is deepest.

Summary with regard to the Alpine Lakes.—And now, in reviewing the subject of the origin of the lakes of Switzerland and North Italy, I would remark—

[12] There are other well known lakes dammed up by the moraine of this great glacier.

1st. That each of the great lakes lies in an area once covered by a vast glacier. There is, therefore, a connexion between them which can scarcely be accidental.

2nd. I think the theory of an area of *special subsidence* for each lake untenable, seeing no more proof for it in the case of the larger lakes than for the hundreds of tarns in perfect rock-basins common to all glacier-countries, present or past, and the connexion of which with diminished or vanished glaciers I proved originally in *The Old Glaciers of North Wales*. In the Alps there is a gradation in size between the small mountain-tarns and the larger lakes.

3rd. None of them lie in lines of *gaping fracture*. If old fractures ran in the lines of the lakes or of other valleys, and gave a tendency to lines of drainage, they are nevertheless, in the deep-seated strata, exposed to us as close fractures now, and the valleys are valleys of erosion and true denudation.

4th. They are none of them in simple synclinal basins, formed by the mere disturbance of the strata after the close of the Miocene epoch: nor,

5th, Do they lie in hollows of common watery erosion; for running water and the still water of deep lakes can neither of them excavate profound basin-shaped hollows. So deeply did Playfair, the exponent of the Huttonian theory, feel this truth, that he was fain to liken the Lake of Geneva to the petty pools on the New Red Marl of Cheshire, and to suppose that the hollow of the lake had been formed by the dissolution and escape of salts contained in the strata below.

6th. But one other agency remains—that of ice, which, from the vast size of the glaciers, we are certain must have exercised a powerful erosive agency. It required a solid body, grinding steadily and powerfully in direct and heavy contact with and across the rocks, to scoop out deep hollows, the situations of which might either be determined by unequal hardness of the rocks, by extra weight of ice in special places, or by accidental circumstances, the clue to which is lost, from our inability perfectly to reconstruct the original forms of the glaciers.

7th. It thus follows that, valleys having existed giving a direction to the flow of the glaciers ere they protruded on the low country between the Alps and the Jura, these valleys and parts of the plain, by the weight and grinding power of ice in motion, were modified in form, part of that modification consisting in the excavation of the lake-basins under review.

In connexion with this point, it is worthy of remark that glaciers, many of them very large in the modern sense of the term, on the south side of the Vallais (excepting those of Mont Blanc), and the large glaciers on the south side of the Oberland, all drain into the Lake of Geneva; those on the north of the last-named

snow-field, also large glaciers, are drained through the Lakes of Breinz and Thun. These, among the largest existing glaciers of the Alps, are only the shrunken tributaries of the greater glaciers that in old times filled and scooped out the basins of the lakes. The rest of the lakes, as already stated, are in equally close connexion with the old snow-drainage of glacier-regions on the grandest scale,—all of them, excepting those of Neuchâtel, Bienne, and Morat, lying in the direct course of glaciers filling valleys that extend right into the heart of the mountains.

8th. Most of the lakes are broad or deep according to the size of the glaciers that flowed through the valleys in which they lie, this general result being modified according to the nature of the rock and the form of the ground over which the glacier passed. Thus, the long and broad Lake of Geneva, scooped in the Miocene lowlands, is 984 feet deep, and over its area once spread the broad glacier of the Rhone. Its great breadth and its depth evince the size of the glacier that overflowed its hollow. The lake of Constance, lying in the same strata, and equally large, is 935 feet deep, and was overspread by the equally magnificent glacier of the Upper Rhine. The Lakes of Maggiore and Como, deepest of all, lie in the narrow valleys of the harder Secondary rocks of the older Alps; and the bottom of the first is 1992 feet, and the latter 1043 feet, below the sea-level. Both of these lie within the bounds of that prodigious system of glaciers that descended from the east side of the Pennine Alps and the great ranges north and south of the Val Tellina, and shed their moraines in the plains of Piedmont and Lombardy. The depth of the lakes corresponds to the vast size and vertical pressure of the glaciers. The circumstance that these lakes are deeper than the level of the sea does not affect the question, for we know nothing about the absolute height of the land during the Glacial period.

The lakes of Thun and Brienz form part of one great hollow, more than 2000 feet deep in its eastern part, or nearly 300 feet below the level of the sea. They lie in the course of the ancient glacier of the Aar, the top of which, as *roches moutonnées* and striations show, rose to the very crests of the mountains between Meyringen and the Grimsel.

The Lake of the Four Cantons is imperfectly estimated at only 884 feet in depth; but here we must also take into account the great height and steep inclines of the mountains at its sides. The Lake of Zug, 1311 feet deep, lies in the course of the same great glacier, the gathering-grounds of which were the slopes that bound the tributaries of the Upper Reuss and the immense amphitheatre of the Urseren Thal, bounded by the Kroutlet, the Sustenhorn, the Galenstock, the St. Gothard, and the southern flanks of the Scheerhorn.

The lesser depths (660 feet) of the Lake of Zurich were hollowed by the smaller but still large glacier that descended the valley of the Linth.

This completes the evidence.

Lakes of the Northern Hemisphere generally.—I shall now make a few remarks on the bearing of this subject on the glacial question generally.

It is remarkable that in Europe and North America, *going northward*, lakes become so exceedingly numerous, that I have been led to suppose the existence of some intimate connexion between their numbers and the northern latitudes in which they occur.

Let any one examine the map of North America, and he will find that, from the Atlantic coast to the St. Lawrence, through New Hampshire, Vermont, the north of the state of New York, Maine, Nova Scotia, New Brunswick, Gaspé, and Newfoundland, the whole continent is strewn with lakes. North of the St. Lawrence and the great lakes, as far as the Arctic Ocean, the same sprinkling of unnumbered lakes over the entire face of the country is even more remarkable; and it is a curious circumstance that a large part of this vast area is so low and undulating, that some of its lakes drain two ways—towards the North Sea and the Gulf of Mexico, or towards the North Sea and the North Atlantic. This vast country, about as far south as lat. 40°, shows, almost universally, marked signs of the strongest glacial action, in the *moutonnée* forms, polish, and constantly recurring striation of the rocks. I have only seen a few of the above-mentioned lakes south of Lake Ontario; but I have closely questioned that able observer, Dr. Hector, who has examined the country north and west of the great American lakes, and he informs me that, though unable to account for it, he was struck with the circumstance that so many (he thought he might say *all*) of the smaller lakes are in *rock-basins*. I connect this circumstance with the universal glaciation of the country, still evinced on the grandest possible scale by every sign of ancient ice. These signs, I now believe, are far too universal and unvarying in their general directions to have been produced merely by floating ice, though in part of the glacial history of the continent floating ice has undoubtedly left large traces. But the lake-basins could only, I believe, have been scooped out by true continental glacier-ice, like that of Greenland; for the lakes are universal in all the ice-worn region.[13]

[13] Since this memoir was written, I have conversed on the subject with Sir Wm. Logan, Director of the Geological Survey of Canada, who not only agrees in my views with respect to the origin of American lakes in general, but also believes that the great American lake-basins may have been scooped out by the same means. They are all true rock-basins, in areas occupied by comparatively soft rocks surrounded by harder strata. Given sufficient time, I see no difficulty in this

On the eastern side of the Atlantic, Wales, Cumberland, many parts of Ireland, the North Highlands, and some of the Western Isles are also dotted with unnumbered lakes and tarns. All of these are well-glaciated countries, both high and low; and for Wales and many parts of Scotland, I can answer that by far the greater proportion of these lakes lie in rock-basins of truly glacial origin.[14]

Loch Lomond and Loch Katrine, probably, like the greater lakes of Switzerland, are of the same kind, being merely large cases of glacier-erosion, though in the case of the former it may be that the alluvial deposits on the banks of the Leven prevent its being invaded by the tide. Its islands are mere *roches moutonnées*.[15]

In the lowlands of Scotland numerous examples of the same kind of rock-basins occur, some of them certain, others doubtful because of the surrounding drift, which indeed in some cases may be the sole cause of the retention of the water. Notable examples of both kinds occur in the lowlands of Fife and Kinross, and of true rock-basins in the Cleish and Ochil Hills, as for instance Loch Glow, Dow Loch, and the two Black Lochs, and more doubtfully Loch Lindores.

I have not yet had an opportunity of visiting the Scandinavian peninsula, which, geologists are aware, is through all its length and breadth, one of the most wonderfully glaciated countries in the world. On the west, descending from the great chain, striated *roches moutonnées* plunge right under the deep fiords; and on the east, in Sweden, all between the mountains and the Baltic, round the Gulfs of Bothnia and Finland, and up to the North Sea, the whole country is covered with a prodigious number of lakes, just like North America, the Lewes, and the North Highlands of Scotland. The intense glaciation which all of these countries have undergone, their similarity, and what I believe to be the intimate connexion of such crowded lakes with the movement of ice, induce me to believe that in Sweden also a great number of the lake-hollows must be true rock-basins

view, to which I inclined while writing this paper, but refrained from stating it, considering that most readers would think it too strong, and thus that in general opinion I might damage the whole theory. Sir William says that the arrangement of the strata proves that the great lakes do not lie in areas of special subsidence.

[14] See 'The Old Glaciers of North Wales.' When I published my account of these glaciers, I was too timid to include the Lakes of Llanberis, Llyn Ogwen, Llyn Cwellyn, and some others of the larger lakes in this category. I now feel convinced that they are true rock-basins, and also that the shallower pools of Llyn Llegeirin, Llyn Felin-y-nant, and others in Anglesea had the same origin. The horizontal striations far up the side of Carnedd Dafydd, by Llyn Ogwen, were probably made by a glacier of immense thickness during the first great glacier-period, preceding the deposition of the stratified drift.

[15] When the lake was low, I have seen in Loch Lomond ice-striated surfaces of rock just above the water, the striations running in the direction of the length of the lake.

scooped out by the passage of glacier-ice into the Baltic area. Furthermore, as the glaciated sides and bottoms of the Norwegian fiords and of the saltwater lochs of Scotland seem to prove, each of these arms of the sea is merely the prolongation of a valley down which a glacier flowed, and was itself filled with a glacier; for the whole country was evidently, like the north of Greenland, moulded by ice. In parts of Scotland, some of these lochs being deeper in places than the neighboring open sea, I incline to attribute this depth to the grinding power of the ice that of old flowed down the valleys, when possibly the land may have been higher than at present.[16] It may, however, only arise from unequal deposition of detritus. If the former view be admitted, raise the land so as to lay bare the surrounding ocean-bottom, and in some respects of levels and depth they become approximately the counterparts of the deeper narrow lakes of Switzerland and North Italy, glaciers bounded by mountains having flowed through both, and debouched upon the plains beyond.

The Glacial Theory.—Furthermore, considering the vast areas over which the phenomena described are common in North America and Europe, I believe that this theory of the origin of lake-rock-basins is an important point, in addition to previous knowledge, towards the solution of the glacial theory; for I do not see that these hollows can in any way be accounted for by the hypothesis that they were scooped by floating ice.[17] An iceberg that could float over the margin of a deep hollow would not touch the deeper recesses of the bottom. I am therefore constrained to return, at least in part, to the theory many years ago strongly advocated by Agassiz, that, in the period of extremest cold of the Glacial epoch, great part of North America, the north of the Continent of Europe, great part of Britain, Ireland, and the Western Isles,[18] were covered by sheets of true glacier-ice in motion, which moulded the whole surface of the country, and in favorable places scooped out depressions that subsequently became lakes.

This was effected by the great original glaciers (probably connected with the origin of the *unstratified* boulder-clay) referred to in my memoir on the glaciers of North Wales,[19] but the magnitude of which I did not then sufficiently estimate. The cold,

[16] But this is not essential, unless the lochs are so deep that the ice must have been floated up before reaching the deeper parts.

[17] I do not in any way wish to deny that much of the glaciation of the lower countries that came within the limits of the Drift was effected by floating ice on a large scale, which must have both polished and striated the rocks along which it ground. I have, with other authors, described this in various memoirs. But the two sets of phenomena are distinct.

[18] The Lewes is covered by small lakes.

[19] Quart. Journ. Geol. Soc. Lond., xviii, 371.

however, continued during the depression of North Wales and other districts beneath the sea, when they received the *stratified* erratic drift; and glaciers not only did not cease at this time of depression, but were again enlarged during the emergence of North Wales and other countries, so as to plough the drift out of many valleys. These enlarged glaciers, however, bore no comparison in size to the great original sheets of ice that converted the North of Europe and America into a country like North Greenland. The newer development of glaciers was strictly local. Amelioration of climate had already far advanced, and probably the gigantic glaciers of Old Switzerland were shrinking into the mountain-valleys.

Finally, if this be true, I find it difficult to believe that the change of climate that put an end to this could be brought about by mere changes of physical geography.[20] The change is too large and too universal, having extended alike over the lowlands of the Northern and the Southern Hemispheres. The shrunken or vanished ice of mountain-ranges is indeed equally characteristic of the Himalaya, the Lebanon, the Alps, the Scandinavian chain, the great chains of North and South America, and of other minor ranges and clusters of mountains like those of Britain and Ireland, the Black Forest, and the Vosges.

[In the *Philosophical Magazine* for November last, Prof. Ramsay has published an article "On the Excavation of the Valleys of the Alps," called out by some discussion of his views, in which he concludes as follows:—EDS.]

"No better proof could be required that in great part the valleys of the Alps were approximately as deep before the glacial epoch as they are at present; and I believe, with the Italian geologists, that all that the glaciers as a whole effected was only slightly to deepen these valleys and materially to modify their general outlines, and, further (a theory I am alone responsible for), to deepen them in parts more considerably when, from various causes, the grinding power of the ice was unusually powerful, especially where, as in the lowlands of Switzerland, the Miocene strata are comparatively soft. But for details on this point I must refer to my memoir in the Journal of the Geological Society."

[20] It has been suggested to me by Dr. Sibson that the prodigious waste of the Alps by the gradual disintegration and diminution of the upper snow-fields, witnessed by the great moraines of North Italy and other phenomena, must have tended to lessen the glaciers. This is true, but, as he also believes, it is not of itself enough to account for the shrinking of the ice into the higher valleys where it is now alone found.

ART. XXXIV.—*Lucernaria the Cœnotype of Acalephæ;* by Prof. HENRY JAMES CLARK, of Harvard University, Cambridge.[1]

THE present communication is a mere sketch of a most thorough and exhausting anatomy of Lucernaria, which I have illustrated by numerous plates, and which I propose to publish in an extended memoir, in connection with some considerations upon the general morphology and systematic relations of Acalephæ. I have been engaged during the whole of the past year upon the organical and histological anatomy of this animal, in order to determine what are its relations to Radiata in general, and to Acalephæ in particular. I have had abundant materials for study, inasmuch as this species of Lucernaria is a very common inhabitant of our shores, wherever the eel-grass, *Zostera marina*, grows. Almost invariably Lucernaria is to be found upon the Zostera, and very rarely upon any other plant. It may be obtained from the last of August, when it is most frequently met with in a young state, until the last of June, at which time the young ones of the autumn season have developed to full-grown animals. In an adult state it measures nearly an inch across the disc, exclusive of the tentacles, and about the same in height. It varies in color from green, which is the most common tint, to deep olive; from light yellow to reddish brown, or from light violet to the deepest purple. In form it is octagonal, and most frequently it so comports itself that the four sides opposite the bifarious genitalia are shorter than those alternating with them, but frequently the same individual reverses the order of things, and the latter become either as short, or even shorter, than the first. From this we infer that the specific differences, based upon the approximation of the bunches of tentacles, two and two, are entirely erroneous, as this obtains in all octagonal Lucernarians, in a greater or less degree. As these animals are very sensitive and irritable, they contract upon the least disturbance; and, as the muscular system is most highly developed in the region which lies about the four partitions of the disc, it is most natural that when the creature contracts it should draw the two halves of the genitalia and the bunches of tentacles together more closely here than at the alternate quarters; hence arises the frequently-observed quadrate outline of the disc. Again, in regard to another feature oftentimes employed to discriminate between different species or even groups, I would say that the absence of auricles alone, without other differences in the animal, does not indicate a specific difference from those individuals possessing them, but rather an accidental

[1] From the Proceedings of the Boston Society of Natural History, for March 19th, 1862; with additions and notes by the author.

atrophy of these organs; and that this fact is to be classed in the same category as the occasional development of one of the tentacles into a semiauricular body. I have always noticed that individuals in such a condition have an unnatural appearance; that they are not so lively as the others, and appear to be diseased.[2] I believe this species to be identical with *L. auricula*[3] of the English coast. The most characteristic figure that I know of, although unsatisfactory, is in Gosse's little book, *The Aquarium*.[4]

In order to contrast the structure of Lucernaria with that of the Steganopthalmatan Medusæ, and, moreover, in order that I may not complicate matters, I will compare it, organ for organ and part for part, with one of our most common medusæ, *Aurelia flavidula* Agassiz. The aboral side, which corresponds to the so-called dorsal region of other Acalephæ, projects at the apex into a moderately long columnar body, usually called the peduncle of Lucernaria. With the exception of the four equidistant channels and the four muscular cords which alternate with them, the peduncle is a solid gelatiniform mass, covered by the outer wall. This gelatiniform substance also constitutes the bulk of the disc, filling the entire space between the outer wall and the inner or lining wall of the digestive cavity, and is directly continuous with that in the peduncle. In Aurelia, Cyanea, and other Acalephs, this substance appears like an amorphous gelatiniform or semicartilaginous mass, with a few irregular cells scattered here and there;[5] but in Lucernaria it

[2] I have found such specimens most frequent at that time of the year which is the breeding season of our common shore-crab,—*Cancer* (*Platycarcinus*) *irroratus*, —when it comes up out of deeper water, and is most abundant and active. At first, only now and then, I found a Lucernarian with one or two auricles bitten off; but later it was common to find specimens with all the auricles nipped, and nothing but a small portion of their base or a mere scar left to indicate their former presence. The moment a Lucernarian is touched by a crab it jerks its tufts of tentacles inward, but the reverted auricles are left exposed, and all the more prominent by the act than usual, and a conspicuous morsel for the predaceous creature. As the season advances towards summer, the bunches of tentacles also disappear one after another, until it becomes quite common also to find individuals with two, three, or four bunches bitten off; and at the same time specimens become more and more rare, at the last of June, for instance, and finally, by the early part of July it is impossible, by the most diligent search, to find a single specimen. As this happens at the time when the Lucernarians are laying their eggs, it is clear that the destruction of the adult does not necessarily annihilate the race. During the next two months no Lucernarians are to be found, but in the last of August I have collected young ones, much less than $\frac{1}{16}$ of an inch in diameter.

[3] *Haliclystus auricula* H. J. C., Journal Boston Soc. Nat. Hist., March, 1863, page 559.

[4] The original figure by Rathke, *Müll. Zool. Danica*, iv, 1806, pl. clii, although sufficiently correct for identification, can neither be called characteristic nor graceful as far as attitude is concerned.

[5] In June, 1862, I made a careful study of the structure of the gelatiniform substance of *Aurelia flavidula* Ag. There are two kinds of fibro-cellular bodies which pervade the gelatiniform layer. One kind are irregular, dark, conspicuous cells, similar in appearance and size to those of the outer wall of the aboral side, with

has a highly organic structure. Extremely elongate, columnar, cell-like bodies extend in close proximity from the outer to the inner wall, so that, in a section of the thickness of the disc, it appears to be transversely striated. In the peduncle, as a transverse section reveals, these columnar cells are arranged about the axis in peculiar, regular groups; some columns pass from one channel to the next on either side; some diagonally across the axis from one channel to an opposite one, and others extend obliquely from the channel to the muscular cords which alternate with them. This arrangement reminds one of the methodical disposition of the great cells in the body of Pleurobrachia,*

from one to four or five jagged, caudate prolongations projecting in every direction. These are most numerous next the aboral side of the disc, and departing from that region, they become less frequent as we approach the oral side, at which place they are very much scattered. The other kind of bodies are very faint, nucleated, nodose fibres, and form a vast anastomosing net-work, which, like the darker caudate cells, pervades the whole of the gelatiniform mass of the body, from the aboral to the oral side. It resembles elastic tissue very closely. Next the aboral side these fibres trend mostly parallelwise with the outer wall, or at very oblique angles to it; but, passing inwardly, they gradually assume a direction transverse to this, and then, anastomosing less frequently, they become in appearance like slender parallel columns, based upon the double wall in which the chymiferous channels run. Between the latter and the outer wall of the oral side the fibrous bodies are excessively faint, and less frequent, but still continue the trend which they have on the aboral side of the double wall. The peculiarities of these two kinds of bodies are fully described by Max Schultze, *Ueber den Bau der Gallertscheibe der Medusen. Müll. Archiv.*, 1856, p. 311, pl. xi, xii, from observations which he made upon *Medusa* (*Aurelia*) *aurita*, *Rhizostoma Cuvierii* and *R. Aldrovandii*; but in all of them he says the fibres run in every direction. "Sie laufen gestreckt in allen Richtungen, theilen sich häufig und verbinden sich unter einander unter allen möglichen Winkeln." Now in *Medusa* (*Aurelia*) *aurita*, which is very near, if not identical with our Aurelia, *A. flavidula* Ag., it is very probable that these fibres are arranged as in ours, and yet I cannot see how Schultze could have overlooked this arrangement. My observations were made upon perfectly fresh specimens, and without the help of any reagents. In our Lucernarian, and in fact in all the Lucernariæ (see *Journal Boston Nat. Hist. Soc.*, March, 1863) the fibrous bodies do not anastomose, but trend in direct lines from the outer to the inner wall.

* At the time the investigation of the gelatiniform mass of *Pleurobrachia rhododactyla* Ag. was made, I had not in my possession lenses of the proper definition and working distance to make out the histological elements with the requisite care that such excessively transparent bodies demand, and therefore, using inferior lenses, I fell into an error which I am only too glad to correct. Since that time I have obtained one of Tolles' half-inch objectives with an exceedingly sharp definition and an extraordinary working distance; so that I have been enabled to work with perfect freedom upon the living animal, and without injuring its tissues in the least. What formerly I mistook to be the outlines of the walls of enormous cells are in reality *elastic fibres*. The mistaking the fibres for the profile of cell walls does not affect the arrangement in the least, as I formerly described it, and which I have since verified with my new objectives. The elastic fibres assume various forms, according to the degree of expansion or contraction of the animal; sometimes they are perfectly straight and at others they are contracted either in a loose spiral, or retracted into a close coil. This is most easily observed in young specimens. In the young of another Ctenophoran, viz., *Bolina alata* Ag., about $\frac{1}{12}$ of an inch in diameter, at which size its proportions, shape, the considerable depth of the tentacular sockets, and the length of its tentacles render it remarkably like a Pleurobrachia, the elastic fibres are very few, but quite conspicuous, and have a peculiar mode of branching. Single fibres extend radiatingly from the corners of the stomach; when about half

as I have described them in Prof. Agassiz's third volume of his "Contributions to the Natural History of the United States." In the oral or lower side of the disc of Aurelia, the gelatiniform substance has the same structure as in the aboral side, while in Lucernaria, although it has all the regularity in the disposition of its components that obtains in the aboral side, yet it possesses a totally different nature, as I will describe hereafter in connection with the muscular system.

From the middle of the base of each of the four flat sides of the quadrate proboscis, a light streak, which has the deceptive appearance of a radiating canal, passes in a direct line nearly to the border of the disc; this is the line along which the oral and aboral floors of the disc unite, and form a solid partition, by which the digestive cavity is divided into four broad chambers, which communicate with one another at the inner or proximal ends, about the base of the proboscis, and also at the outer or distal ends through the narrow passage between the terminus of the partition and the edge of the disc. In the peduncle there are four equidistant broad tubes, which merge into one cavity at its base, and correspond in position to the four chambers of the digestive cavity. The grouped tentacles which occupy the eight corners of the disc are hollow, as, likewise, are the auricles, and communicate openly and directly with the digestive cavity. This is all that constitutes the chymiferous circulatory system of Lucernaria. In Aurelia we have radiating canals at the points corresponding to the partitions of Lucernaria, as well as in the intermediate sections.

In Aurelia, the genitalia are four single circular organs, one of each being placed opposite the flat side of the proboscis; whereas in Lucernaria each genital is a double organ, the halves of which have a peculiar shape, and are situated respectively one on each side of the partition, and extend along the inner face of the oral floor of the disc from the base of the proboscis to the extreme limits of the corners of the disc, where they almost touch the bases of the tentacles. Across the proximal end of each partition, triple or quadruple rows of slender digitiform bodies extend each way for a considerable distance along the border of each half of a genital, thus forming the common

way to the surface of the body, each fibre forks two or three times, and then one prong goes to each of the two nearest longitudinal chymiferous tubes, and the third one extends to the base of the deep tentacular socket. This is the general arrangement at this age, although occasionally one of the prongs of the fork is absent, or only partially developed. Sometimes each prong forks again, at a narrow or wide angle. From the tentacular sockets fibres extend also to the surface midway between the mouth of the former and the adjacent longitudinal chymiferous tube. So few are all the fibres, however, that with a casual glance they might be mistaken for light, unimportant streaks here and there, instead of such methodically arranged bodies.

appendages of the two, and clearly indicating their *unity*.' Each half of a genital has a peculiar form, which may be represented by an inequilateral triangle whose longest side extends nearly in a straight line from the inner end of the partition to the tentacles, and the two other sides, slightly curving outwardly and meeting at a very broad angle, form the rest of the outline. In the adult, the longest side of the triangle is to its height as two to one. This feature, alone, has a degree of speciality which raises these organs in rank above all others of their kind among Acalephæ; but when we examine their components, we find an unlooked-for structure, hitherto unknown among Acalephæ. What appear, to the naked eye, to be eggs of enormous size, are really little pouches, which contain either numerous eggs or matrices of spermatic particles, according as the individual is male or female. Each pouch, or *genital saccule*, as it may be called, projects freely into the digestive cavity, and is attached by a very short and rather narrow neck to the inner wall of the oral floor of the disc. This constitutes another step in the specialization of these organs, but does not complete the process. At the base of each genital saccule, and on that side which faces toward the proboscis, there is a small aperture, which leads to the interior, where there is a considerable cavity. This cavity is formed by the lateral inversion of the single wall of the saccule upon itself, and the constriction of the wall about the entrance to the chamber. The eggs or spermatic material[8] are enclosed in saccular folds of the wall of this chamber, into which they fall when mature, and pass thence outwardly through the lateral outlet at the base of the saccule. One may see at a glance that this is a type of the reproductive organs not to be found among the other Acalephæ.

In Aurelia, the generative products, whether eggs or spermatozoa, lie immediately beneath the *outer wall*, and imbedded in the muscular layer which extends throughout the length and breadth of the oral face of the disc, as I have described it in the fourth volume of Professor Agassiz's "Contributions." Between

[7] In the family Cleistocarpidæ, as I have recently characterized it (*Journal Boston Nat. Hist. Soc.*, March, 1863), the genital halves are directly united to each other, so as to form a continuous organ across the proximal end of the partition; thus there can be no doubt that there are but four genitals in Lucernariæ, and not eight, as described by various authors.

[8] The spermatic particles have an elongate-cordate body, from the broad end of which an excessively long tail-like filament trails in broad curves as it swims; at the pointed end are attached two exceedingly delicate filaments, which are in constant motion, bending and coiling, or stretching in every direction, as if they were the tactile organs of an Euglena or some other similar Infusorian. These pseudo-probosces defy detection with ordinary objectives; in fact, to determine their presence with certainty requires very careful manipulation of such objectives as have the most accurate defining power, and which are to be obtained only from our best makers. The spermatic particles of our common Echinus, *E. granulatus*, also possess a double pseudo-proboscis.

the muscular layer and the inner wall, which forms the immediate parietes of the digestive cavity, a thick layer of gelatiniform substance intervenes, and its presence naturally suggests the inquiry, how are the eggs or sperm to escape into the digestive cavity, as they are known to do? The spermatic particles I have observed frequently escaping directly through the outer wall into the ocean, and I have seen them, with the broadest end out, projecting like bundles of hairs from the cavity of the matrix through the apertures in the outer wall. When the reproductive material is fully ripe, the inner wall, with the gelatiniform layer, and the muscular layer as far as it includes the material in question, splits off from the outer wall along two lines corresponding to the two borders of the generative organ, and hangs loosely, in ribbons, in the digestive cavity. From the newly-formed raw face of these ribbons the eggs or spermatic particles escape into the main chamber of the disc. This I take to be the universal rule, and such the type of genitalia among all Steganophthalmata; a structure totally unlike that of Lucernaria, in which the *inner wall* alone is concerned in the highly complicated reproductive organs.

Passing now to the consideration of the *muscular system*, I will call your attention to the four white, slender columns which alternate with the four dark tubes which are imbedded in the gelatiniform substance of the peduncle. Sars was the first to indicate the true nature of these columns, and he rightly called them muscular cords. They extend from the base of the peduncle to the base of the proboscis, coursing along just beneath the outer wall, but still within the gelatiniform substance, until they reach the upper third of the peduncle, and then gradually approximating the axial line, they meet the inner wall of the disc just below the base of the proboscis, and thence they pass along still beneath this wall, for a short distance, and, finally *each one enters the oral side of the disc* at the inner or axial end of the partition. At this point, each muscular column expands and forms a fan-shaped layer just beneath the outer wall, and extends laterally so as to occupy the whole space between the two halves of a genital. At the distal end, this layer diverges right and left of the partition into a broad muscular band which borders the disc, and, eventually, is distributed in ridges or cords beneath the outer wall of the tentacles and the auricles. At the inner end of the partition, the muscular layer also passes into the base of the proboscis, and forms a stratum immediately beneath the outer wall. At four equidistant points, alternating with the partitions and genitals, and opposite the four corners of the proboscis, there is a weaker muscular layer, which occupies the same relative position in regard to the outer walls as does the stronger system of muscles first mentioned. On the one hand,

it passes into the marginal muscular band, and on the other it enters the corners of the proboscis, and forms a layer in common with the one extending from the partitions. By these alternating stronger and weaker divisions of the muscular layer, the disc is relieved of the sameness which prevails in the muscular system of the Steganophthalmata, and we have indubitable proofs of a higher degree of specialization than in the latter order, where the unvarying repetition of similar divisions all around the disc unmistakably indicates inferiority. Moreover, in addition to this, we have a peculiar specialization of the gelatiniform layer, which is embraced by the outer and inner walls of this floor, or rather between the muscular layer and the inner wall; instead of repeating, as occurs in Aurelia, the peculiarities of the gelatiniform layer of the aboral floor, it has a totally different appearance and consistency, and an almost unlimited degree of expansion and contraction. In the tentacles it occupies a very deep space between the outer wall, or rather the muscular layer, and the inner wall. In this latter respect, Lucernaria is again peculiar, since in addition to the muscular layer, which alone is present in the young, it develops this gelatiniform layer,—the *musculo-gelatiniform layer* as I propose to call it,—the like of which does not exist in the tentacles either of Steganophthalmata or Gymnophthalmata. In the auricles, we have also a specialization peculiar to Lucernaria; for, in addition to the pigment eye-spot which is imbedded in the base of the oral face of these bodies, the auricles, which in the young cannot be distinguished from the tentacles, gradually thicken the outer wall as age advances, and peculiar, granuliferous, adhesive vesicles are developed between the cells. In the adult, their tentacular nature is almost, or altogether, obliterated, and the swollen outer wall, together with the enormous thickness of the musculo-gelatiniform layer, forms an oval mass, thickly studded with adhesive organs, by which they cling, in a most tenacious manner, to any body which they may touch. These organs, and the base of the peduncle, are the only means of adherence which Lucernaria possesses; although it is true that the tentacles are used, as in Aurelia, for prehension, they are, comparatively, very weak, and can only serve to retain the prey, and never effect the purpose for which the auricles are constructed.* In consideration of the very obvious office of an auricle, I would propose the name *anchor* for it.

* The nettling organs, or lasso-cells, which crowd the globular tips of the tentacles, are of two kinds, and both are imbedded in the intercellular substance which fills the spaces between the columnar cells of the outer wall. One kind consists of an oval, thick-walled vesicle, about $\frac{1}{2000}$ of an inch long, or a little less, one end of which is introverted, and projects, in the form of a stout hollow shaft, along the axis of the cell about four-fifths of its length, and then, rather suddenly thinning into a slender thread which also is hollow, it bends upon itself, returns nearly to the

Were the above-mentioned features in the organism of Lucernaria alone to be taken into account, there could be no hesitation in saying that this genus should be considered as the highest of the class of Acalephæ; because of its highly complicated and specialized gelatiniform mass; the high grade, and the peculiar and distinctive grouping of its muscular system; the definite and bilateral form of the genital organs, as well as their saccular subdivision; the two-fold nature and disposition of the prehensile organs, the tentacles and anchors: and, moreover, that it belongs to an order separate from either orders of Acalephæ, because of the typical elements of its genital saccules, which are altogether different from either the Steganophthalmic or Gymnophthalmic type of genitals; and also on account of the anchors, which have no parallel in all the class of Acalephæ. But there are parts of the Lucernarian organism which are of a lower grade than those of similar nature among the other Acalephæ. I refer, in the first place, to the hydra-like form of Lucernaria, and its comparatively stiff and hydroidal tentacles, evidently indicating

aperture of the cell, and pressing closely against the inner face of the cell wall it forms a close coil which terminates at the end opposite the mouth of the introversion. When the coil of thread is ejected, which is accomplished by sliding through the hollow axial shaft, which in its turn retroverts also, just as the finger of a glove is turned inside out, the whole aspect of the apparatus is changed. The oval cell is considerably diminished in size, and from its aperture the enormously enlarged hollow shaft projects in a straight line; the half of the shaft next the cell is cylindrical, and half as broad as the latter, with a slight expansion where it joins the mouth of the cell; the distal half abruptly expands into an oval form, half again broader than the cylindrical portion, and rapidly tapers into a smooth, trihedral, twisted thread. The oval part of the shaft is endowed with three equidistant spiral rows of setæ, which number about a dozen in each row. The setæ are comparatively large, and in length equal two thirds the broadest diameter of that part of the shaft from which they project. Each row makes but one turn about the shaft, and terminates as if in continuation of the angles of the trihedral thread. There is not the least trace of setæ or projections of any kind upon the trihedral thread, but it continues, with a very gradual taper, perfectly smooth, to the blunt termination. The angles of the thread appear, at first glance, as if they might be spiral rows of setæ, but a most careful and prolonged examination, with one of Spencer's $\frac{1}{4}$-inch objectives, convinces me that they are truly the angles of a twisted trihedral filament. The extent of the thread is from twenty to twenty-four times the length of the cell. The other kind of nettling cell is much more simple in structure, but yet more remarkable. The introverted shaft is very slender, in fact no larger than the rest of the thread; it does not project into the axis of the cylindrico-oval cell, but presses close to the side of the latter, and extends four-fifths of the way to its opposite end, and then bending abruptly upon itself, the thread passes with a long curved sweep nearly to the aperture of the cell, from whence it again returns, with another long sweep, which is repeated eight to ten times, until the inner face of the cell wall is lined by a close coil which winds lengthwise, instead of transversely as it does in the other kind first described. When extended, the thread is from twelve to fourteen times the length of the cell; it offers not the least sign of appendages of any kind, but is simply a smooth, round filament, of uniform thickness throughout, except at the end, where it tapers slightly and terminates in a blunt tip. The cell itself, when retroverted, is sensibly diminished in size, and narrows rapidly into the prolonged filamentary portion. It would seem to be perfectly incontestable that, as the cell diminishes in size with the expulsion of the thread, it forms the propelling power, and, by the contraction of its wall, forces its contents outward.

a typical affinity to the fixed hydroid generation of the Sarsiæ, Bougainvilliæ, Steenstrupiæ, etc. The simple, almost unilocular chymiferous system is hardly more medusoidal, as regards the multiplicity of its subdivisions, than in some of the Tubularians, such as Tubularia and Corymorpha, which are described in Professor Agassiz's fourth volume of his "Contributions." In connection with the hydroid form of Lucernaria, I would also mention the total absence of a veil. This might, at first thought, appear to furnish an argument in favor of the high relations of this genus; but I think it is to be deemed as one of the signs of its inferior connections. However, let us look at the progress of velar development. In the *ephyra* state of all Steganophthalmata, the veil is at one time greatly in the preponderance, when compared with the size of the whole individual; but with growth it gradually becomes less conspicuous, and, finally, in some adult genera of this order, it remains as a mere trace of a veil, or, as in Cyanea and some Rhizostomidæ, it is altogether obscured. Now, it is noteworthy that among the lowest of this order, such as Pelagia, we have a strong resemblance to the ephyra state, and the ephyroid, tongue-like veil is quite prominent; and in Chrysaora it is hardly less so; ascending the scale, we find it yet more inconspicuous in Aurelia, and still more so in Cassiopeæ; and, finally, altogether absent in Cyanea,[10] the highest, in my opinion, of all the Steganophthalmata. Now, one might suppose Lucernaria, in respect to the veil, to be in the same category with Cyanea, which has resorbed its veil; this, however, is not the case, for as I know, from the study of the younger stages of Lucernaria, that it never passes through the veiled phase, it falls short in its development as regards this particular feature of Acalephan morphology. We must take into consideration, also, the eyes, which are found to be as low in point of structure as the merest pigment eye-spot of the Gymnophthalmata.

Thus, in balancing the value of the organisms of this animal, we are inevitably led to the conclusion, on the one hand, that Lucernaria does not stand *as a totality* above all other Acalephæ, nor, on the other hand, does it, by any means, belong below them; and that much less does it affiliate exclusively with the

[10] The ephyra-like appearance of Cyanea is illusory; the lobes about the eyes, comprise not only the original ocular lappets, but also a part of the tentacular margin; in fact one half of each margin on each side of an eye is continuous with the ocular lappet adjacent. The tentacular margin being incurved toward the centre of the disc, the veil must be still farther inward, and very probably the margin of the muscular bands corresponds to it, the two merging into each other. The wide lacunar character of the radiating canals is not a feature of inferiority, as might appear, but represents a continuation of the tendency,—as may be seen in the progressive stages of growth of Aurelia,—to channel out the whole breadth of the disc, until it finally becomes a simple cavity. In Rhizostoma, Stomolphus and Polyclonia, the channeling is less carried out than in Cyanea; in fact, in the former it is but a little beyond Aurelia in this respect.

Gymnophthalmata. The only relation that it possibly can be considered under is that of a *correlation to both types of Acalephæ*, —viz.: to the Gymnophthalmata, including the Siphonophoræ, and to the Steganophthalmata; yet not as a graduated connecting link, which would seem to show that the two orders pass into each other, but as an *ordinal type*, equivalent in value to either of the others, by reason of the peculiar and distinctive morphology of certain of its organs. On this account, Lucernaria is to be considered, and may be designated, as the *cœnotype* (κοινὸς, common) of the Acalephæ. In this respect, it holds such relations to the other two orders of Acalephæ as do the Crinoids to the other orders of Echinodermata; or the Annelidæ to the rest of the Articulata; or the Selachians to the true fishes and the reptiles; but, at the same time, containing organic features which separate each of them as a type from the others.

In order that no confusion may arise here, I would state most explicitly that I do not consider the Ctenophoræ as one of the orders of Acalephæ, but deem them to be a class by themselves, equal in value to either of the classes of Radiata, whether Polypi, Acalephæ, or Echinodermata, and standing next in rank to the Echinodermata. The division of the alimentary system of Ctenophoræ into *two portions*, as among Polypi, is sufficient to separate them from the Acalephæ, since the typical form of the corresponding system in the latter is a *unity;* moreover, the position and peculiar relations of the tentacles of Ctenophoræ are hardly of less importance, in these considerations, as distinctive characters. I cannot conceive that the Ctenophoræ may be included in the same classific type with the Acalephæ without doing violence to correlative ideas such as are expressed in the organism of the former; and much less can I admit that they have the most distant relation to the Polypi, excepting that, like the latter, they are Radiates. The same kind of arguments that have been used to show that Ctenophoræ and Polypi belong to one class might, with equal justice, be advanced to prove that the Acalephæ are Polypi. We must not mistake a similarity for an identity, any more than that the cry of a child would identify it with a cat, because their voices sound alike, and cannot always be distinguished the one from the other by any single faculty of our senses.

The following tabular view presents at a glance the relations of the Lucernariæ to the other orders of Acalephæ, and at the same time indicates the position of the Ctenophoræ among the other classes of Radiata.

Polypi.	Acalephæ.	Ctenophoræ.	Echinodermata.
	Steganophthalmata.		
	Lucernariæ.		
	Gymnophthalmata.		

ART. XXXV.—*On the use of Prisms of Flint Glass and Bisulphid of carbon for Spectral Analysis;* by Prof. O. N. ROOD.

IN a letter to Prof. B. Silliman, Jr., which was published by him in the September No., 1862, of this Journal, I described a new form of bisulphid of carbon prism, provided with compound faces, which corrected the distortion usually attendant on such prisms. I ventured at that time to suggest that large prisms of this kind approached a degree of optical perfection not attainable by the best flint glass prisms yet produced. Some late experiments of Sigmund Merz,[1] one of the successors of Fraunhofer, furnish a confirmation of my opinion, which I certainly did not expect to receive from that particular quarter. In my letter I mentioned the discovery of two new lines in the interior of the line D, which made in all three fine lines that were thus enclosed, one having previously been laid down by Kirchhoff. To effect this, three bisulphid of carbon prisms of 60°, with a flint glass prism of 45° were employed; the sum of the refracting angles was then 225°. Now Merz states that by the use of a number of *glass* prisms, the sum of their refracting angles being 270°, or 45° greater than that employed by me, he discovered a second line in the interior of D, but nothing more; the third line it appears was invisible. This second line observed by him I may remark, in my spectroscope was apparently as strong as that laid down by Kirchhoff, so that it was a matter of some wonder that it had escaped resolution in his hands.

Merz then employed eleven glass prisms, the sum of their refracting angles being 480°; with these he discovered the third line I had previously seen, along with two additional quite fine lines. He therefore describes the line D to consist of: two quite broad lines, (those commonly known,) two of less breadth, and three fine lines.

When we consider that this optician had at his command the best flint glass prisms in the world, and observing telescopes that have hardly ever been surpassed, the argument to be drawn in favor of bisulphid of carbon prisms properly corrected, is I think a strong one; particularly when I mention that the telescopes used by me were the common cheap French article, variously amended to secure an approximation to achromatism.

Farther, according to the observations of Merz, a single *large* glass prism (43 lines) used with a large condensing telescope (34 lines in diameter), shows D resolved into five lines, demonstrating thus the value of *size* in the apparatus; this seems again to me an excellent reason for the use of bisulphid of carbon on the ground of its far greater cheapness.

Peace Dale, R. I., March 10th, 1863.

[1] *Ueber das Farben Spectrum von Sigmund Merz in München. Pogg. Annalen,* Band cxvii, stück 4. (Aus einer vom Hm. Verf. übersandten Abhandlung ein Kunst-und Gewerbeblatt d. polytechn. Vereins für d. Königr. Bayern, Oct. 1862.)

ART. XXXVI.—*On certain Appearances produced by Revolving Discs;* by Prof. O. N. ROOD.

DOVE, some years ago, succeeded in producing a lustrous appearance, by the binocular combination of geometrical figures, executed in black and white, or in complementary colors,[1] and later I showed that *surfaces* without drawings produced the same effect.[2]

In both these cases, two masses of light were continuously presented to the two eyes of the observer. It subsequently appeared to me of interest to examine the effects produced by a more or less rapid alternation of these impressions, and accordingly some experiments were made with this object in view.

A circular disc of white card-board, 9 inches in diameter, with half its surface painted of a dead black, was caused to rotate by clock work at varying rates, while the bright light from a window fell upon it. A stereoscope, from which the ground glass had been removed, was provided with a card-board in which were cut two square apertures, at such a distance asunder that their binocular union could be easily effected, and, while the disc was at rest, the stereoscope was arranged so that through the right-hand aperture some of the white portion of the disc was seen, and through the left-hand aperture a part of the blackened surface. On communicating rotary motion to the disc, a more or less rapid alternation of black and white was the result.

It was found that with slow rates of rotation ($2\frac{3}{16}$ revolutions per sec.) the strength of the lustre was not impaired, and it was just as plainly perceptible with more rapid rates.

But when the disc was made to revolve so fast that its surface seemed covered by a uniform tint of grey, and the so-called flickering had ceased, no lustre in the proper sense of the term could be seen, the appearance being exactly that which is presented to a single eye under similar circumstances.

When a disc of this kind revolves at such a rate as to appear of a uniform tint, the duration of the impression produced on the eye by the white half lasts with undiminished force while the black half is passing before the same eye, so that while the right eye is being objectively impressed by the white surface, the left eye has retained a subjective impression of exactly the same nature and strength; both eyes are then really *in effect* impressed all the time in exactly the same way, and in consequence of this no lustre is perceptible. But when the rates of rotation are lower than that above indicated, a different binocular com-

[1] Farbenlehre, pp. 171 and 177. [2] This Journal, May, 1861.

bination takes place; here, while one eye has objective white light presented to it, the other retina is affected by a rapidly fading subjective impression, so that the two impressions are during most of the time of unequal intensity; the result is lustre.

In this connexion, a remark on the appearance of rotating discs with black and white sectors, when viewed by a single eye, may not be out of place. Let us take for the sake of convenience a disc with seven white and seven black equal sectors, and cause it to revolve by clock work. As long as the rate is quite slow, the figure remains undistorted, but as it is increased to $1\frac{3}{7}$ revolutions per second there is a loss of definition, and directly the appearance becomes a little puzzling; with a higher rate, as for example $4\frac{3}{7}$ per sec., the disc takes on a very remarkable appearance, described by some as flickering, by others as "*glittering.*" To make a little examination of it undisturbed by its surroundings, I cut a circular aperture 2 inches in diameter in a large piece of card-board, and viewed through this with a single eye a portion of the revolving disc. The appearance presented I can describe in no other terms than by calling it lustrous, with rapid variations in the intensity of the light. In this case the strong objective light is seen through the weaker fading subjective impression, and the latter is of course at regular intervals perceived distinct by itself, so that the eye is in effect acted on by two masses of light of unequal intensity, and is also sensible of their separate presence.

A disc of this kind is remarkable in one other respect, viz: that with both eyes it is impossible exactly to locate its surface without reference to the edge or centre. The disc often seems to me to have a depth of some inches, the rapid shifting of the figure not allowing binocular vision to come fairly into action.

Finally, if the disc be made to rotate so rapidly that the surface appears quite uniform, an attentive examination shows that its surface presents an appearance not a little singular, so that if the experiment be properly made, the surface, taken by itself, cannot be located with any degree of precision, the marks ordinarily used are found to be abstracted, and nothing but a mass of light is seen. It much resembles a mass of *luminous air*, if the expression may be allowed.

To ascertain whether this aerial appearance depended in any degree on the rapid alternations of white and black, I colored a smaller disc grey, the same tint in strength with that produced by the sectors of the disc in revolution, and placing both on the same axis made them rotate together. One looked exactly like the other, and hence it is to be concluded, that this aerial appearance is caused solely by the disappearance of everything like markings or texture on the paper.

Peace Dale, R. I., March 10th, 1863.

ART. XXXVII.—*Abstract of Results of a Magnetic Survey of Pennsylvania and parts of adjacent States in* 1840 *and* 1841, *with some additional results of* 1843 *and* 1862, *and a map;* by A. D. BACHE, LL.D., F.R.S., Mem. Corr. Acad. Sci. Paris, Mem. Nat. A. S., Superintendent U. S. Coast Survey.

INTRODUCTION.

IN the years 1840 and 1841, I made a detailed magnetic survey of Pennsylvania and adjacent parts of New York, Ohio and Maryland, determining at a number of stations suitably selected, with regard to the course of the isomagnetic lines, the magnetic declination, dip and intensity; to these I added some dip and intensity observations in 1843, while on a tour through western New York and Canada.

The total number of declination stations is 16, and of dip and intensity stations 48. On assuming the duties of Superintendent of the U. S. Coast Survey, in 1843, I could not find the necessary leisure to work up these observations, although Mr. J. Ruth and Mr. G. Davidson had commenced preparing, under my direction, a partial abstract, confined to dip and intensity observations and to relative results. In the spring of 1862, I availed myself of the service of Charles A. Schott, Assistant in the U. S. Coast Survey, who reduced, under my direction, the observations, discussed the distribution of the three magnetic elements, presenting the latter results also graphically, and prepared this abstract for the press.

In the summer of 1862, Mr. Schott visited six of the stations previously occupied by me, and redetermined the magnetic elements. Three of these stations falling within the scope of the operations of the U. S. Coast Survey, were at the expense of the Coast Survey, the observations at the three Western stations were secured by the liberality of the Secretary of the Smithsonian Institution, who, at the same time, offered to publish the observations and results in the *Smithsonian Contributions to Knowledge.* The observations of 1862 greatly enhance the value of my older operations, and furnish the means of presenting results for two epochs, about 20 years apart, thus, not only giving the most modern values but also determining, by the known secular change of the three elements, any intermediate results.

The fruits of these labors, undertaken for this continent, at a comparatively early period and comprising the three elements, and the whole conducted systematically, with instruments well constructed for the time, will no doubt afford adequate means of watching, hereafter, the secular changes of terrestrial magnetism within the geographical extent of this survey.

The declinations were determined with a new Gambey declinometer belonging to the Girard College: the astronomical observations were made with a sextant and vertical circle and chronometer. (Grant, No. 3861.) The dip was determined with a portable circle by Robinson, and the intensity with Lloyd needles by Robinson, and a magnetic bar and cylinder according to the method described by me in the *American Phil. Trans.*, vol. v, 1837, in which the vibrations are made in a rarefied medium.

The full paper, with records, will shortly be printed in the *Smithsonian Contributions to Knowledge.*

Abstract of results of Declinations, observed in Pennsylvania and adjacent States in 1840–41.

These observations were made with a Gambey declinometer belonging to the Girard College.

One division (small) of the scale was found equal to 14″·54, as determined in 1844 at Sandy Hook by Lieut. G. M. Bache. (See *Coast Survey Records.*) 1 large division = 60 small divisions.

The observations were made with telescope *direct*, with slit to the right hand or *E.*, and with telescope *inverted* with slit to the left or *W.*; also with needle *direct* or hairs *up*, and with needle *inverted* or hairs *down*. With needle north, W. readings are +, E. readings −; with needle south, W. readings are −, E. readings +.

Recapitulation of Results for Magnetic Declination, 1840.

1. Harrisburg, Penn.,	July 25,	3° 12′·5	W.
2. Huntingdon, "	July 30,	1 52 ·3	"
3. Homewood, near Pittsburg,	Aug. 10,	0 08 ·0	"
4. Johnson's Tavern, near Brownsville,	Aug. 17,	0 25 ·2	W.
5. Irwin's Mill, near Mercersburg,	Aug. 24,	0 54 ·4	"
6. Baltimore, Md.,	Aug. 27,	2 16 ·5	"

Recapitulation of Results for Magnetic Declination, 1841.

1. Philadelphia, Penn.,	July 20 and Nov. 1,	3° 53′·7	W.
2. Easton, "	" 23,	3 38 ·0	"
3. Williamsport, "	" 28,	3 31 ·2	"
4. Curwinsville, "	Aug. 1,	1 45 ·1	"
5. Mercer, "	" 4,	0 51 ·2	E.
6. Erie, "	" 9,	0 30 ·0	W.
7. Dunkirk, N. Y.,	" 12,	0 52 ·5	"
8. Ellicottsville, "	" 14,	2 35 ·7	"
9. Bath, "	" 19,	3 31 ·4	"
10. Silver Lake, Penn.,	" 23,	4 30 ·2	"

Recapitulation of observed Latitudes, 1841.

Williamsport, Penn.,	41° 14′·0
Curwinsville, "	40 57 ·7
Mercer, "	41 13 ·8
Erie, "	42 07 ·5

Dunkirk,	N. Y.	42 29·3
Ellicottsville,	"	42 18·1
Bath,	"	42 20·8
Silver Lake,	Penn.,	41 56·6

Comparison of Declination for secular change. Results of 1840-41 *and of* 1862.

				1862. (Schott.)	Annual Increase.
Philadelphia, Girard College,	July & Nov.	1841	3° 53′·7 W.	5° 00′·0 W.	3′·2
Harrisburg,	July,	1840	3 12·5 "	3 44·5 "	1·5
Williamsport,	"	1841	3 31·2 "	4 25·7 "	2·6
Johnson's Tav. near Brownsville,	Aug,	1840	0 25·2 "	1 13·6 "	2·2
Erie,	"	1841	0 30·0 "	1 33·0 "	3·0
Bath,	"	1841	3 31·4 "	4 47·9 "	3·6
				Mean,	2·7

Harrisburg was occupied in July 1862, and all the other stations of 1862 in August.

	Longitude.			
	By Chronom.	From Lake Survey.	Previously adopted.	Final adopted.
	° ′	° ′	° ′	° ′
Williamsport,	77 01·3		77 03·5	77 02
Curwinsville,	78 36·6		78 35	78 36
Erie,[1]	80 12·5	80 05	80 06	80 06
Dunkirk,[2]	79 27·0	79 22	79 22	79 23
Ellicottsville,	78 46·6		78 42	78 44
Silver Lake,	75 59·3		76 05	76 02
Milford,	74 53·1		74 50	74 51·5

Distribution of the Magnetic Declination for the epoch 1842·0.

From the comparison of observations for secular change, we have:

From the preceding 6 stations the average annual increase 2′·7. At Toronto (between 1845 and 1855) 2′·3 (see vol. iii of the *Toronto Observations*).

General table of results referred to the common epoch 1842·0.

No.	Station.	Date.	Observed decl W.	Re'd to epoch.	Declination 1842·0.
			° ′	′	° ′
1	Harrisburg,	1840, July 25	3 12·5	+4·0	3 16 5
2	Huntingdon,	" " 30	1 52·3	"	1 56·3
3	Near Pittsburg,	" Aug. 10	0 08·0	"	0 12·0
4	Near Brownsville,	" " 17	0 25·2	"	0 29·2
5	Near Mercersburg,	" " 24	0 54·4	"	0 58·4
6	Baltimore,	" " 27	2 16·5	"	2 20·5
7	Philadelphia,	1841, July 20 / Nov. 1	3 53·7	+0·7	3 54·4
8	Easton,	" July 23	3 38·0	+1·3	3 39·3
9	Williamsport,	" " 28	3 31·2	"	3 32·5
10	Curwinsville,	" Aug. 1	1 45·1	"	1 46·4
11	Mercer,	" " 4	−0 51·2	"	−0 49·9
12	Erie,	" " 9	0 30·0	"	0 31·3
13	Dunkirk,	" " 12	0 52·5	"	0 53·8
14	Ellicottsville,	" " 14	2 35·7	"	2 37·0
15	Bath,	" " 19	3 31·4	"	3 32·7
16	Silver Lake,	" " 23	4 30·2	"	4 31·5

[1] Colton's Map, 80° 10′.

[2] Ibid., 79° 22′·5.

No.	Station.	Latitude.	Longitude.	Decl. W. 1842·0.
		°	°	°
1	Harrisburg,	40·27	76·88	3·27
2	Huntingdon,	40 51	78·03	1·94
3	Near Pittsburg,	40·47	79·99	0·20
4	Near Brownsville,	39 99	79·80	0·49
5	Near Mercersburg,	39·78	77·93	0 97
6	Baltimore,	39·30	76·61	2·34
7	Philadelphia,	39·97	75·17	3·89
8	Easton,	40·70	75·25	3 65
9	Williamsport,	41·23	77·03	3·54
10	Curwinsville,	40·96	78·60	1·77
11	Mercer,	41·23	80 27	−0·83
12	Erie,	42·13	80·10	0·52
13	Dunkirk,	42·49	79·38	0 90
14	Ellicottsville,	42·30	78 73	2 62
15	Bath,	42 35	77·35	3 55
16	Silver Lake,	41·94	76·03	4·52
	Mean,	40·98	77·95	2·08

The small extent of the survey, as well as the comparatively small number of observations, will not permit the introduction of curvature in the isogonic lines, they are therefore treated as straight lines. This assumption also serves for the recognition of any local disturbances as indicated by the differences of observed and computed values.

Let

$$D = +2°·08 + x\,dL + y\,dM \cos L,$$

where

$$dL = \text{lat.} - 40°·98$$
$$dM = \text{long.} - 77\,·95.$$

The 16 conditional equations have been formed and values of x, y and D found from the normal equations are as follows:

$$x = +0·5102$$
$$y = -1·206$$
$$D = +2°·08 + 0·5102\,dL - 1·206\,dM \cos L.$$

A comparison of the observed and computed declinations shows the necessity of introducing a term involving $dL\,dM \cos L$; this has been done, and the solution of the normal equations gives us the following expression.

$$D = +2°·14 + 0·513\,dL - 1·231\,dM \cos L - 0·203\,dL\,dM \cos L.$$

Comparison of observed and computed values.

Stations.	Observed Declination.	Computed Declination.	Observed − Computed.
	°	°	′
Harrisburg,	3 27	+2·67	+36
Huntingdon,	1·94	1·82	+07
Near Pittsburg,	0·20	0 13	+04
Near Brownsville,	0·49	0·16	+20
Near Mercersburg,	0·97	1·54	−34
Baltimore,	2·34	2·21	+08

Table continued.

Stations.	Observed Declination.	Computed Declination.	Observed − Computed.
	°	°	′
Philadelphia,	3 89	3·81	+05
Easton,	3·65	4·41	−46
Williamsport,	3·54	3·16	+23
Curwinsville,	1·77	1·51	+16
Mercer,	−0·83	0·04	−52
Erie,	0·52	0·44	+05
Dunkirk,	0·90	1·29	−23
Ellicottsville,	2·62	1·96	+40
Bath,	3·55	3·50	+03
Silver Lake,	4·52	4 66	−08

The probable error of any single representation is ±19′·4.
The curves of 0°, 2°, 4°, pass through the following positions:

0°	Lat. 41° 00′	Lat. 42° 30′	Lat. 39° 30′
	Long. 80 15	Long. 80 33	Long. 79 54
2°	Lat. 41° 00′	Lat. 42° 30′	Lat. 39° 30′
	Long. 78 07	Long. 78 46	Long. 77 05
4°	Lat. 41° 00′	Lat. 42° 30′	Lat. 39° 30′
	Long. 75 56	Long. 76 59	Long. 74 17

These curves have been finally adopted.

Distribution of the Magnetic dip, and construction of the isoclinal lines, for 1842.

For the more convenient application of the usual analytical expression for the representation of the observed dips and for their interpolation, the stations have been divided into six groups, as follows:

No.	Group I.	Latitude.	Longitude.	Date.	Observed dips.
		° ′	° ′		° ′
1	Philadelphia,[3]	39 58·4	75 10·0	Feb. 1842	71 57·1
2	Doylestown,	40 18	75 10	July 1841	72 23·1
3	Easton,	40 42	75 15	" 1841	72 39·0
4	Reading,	40 19	75 55	" 1840	72 32·2
5	Frenchtown,	39 35	75 51	Aug. 1840	71 40·2
6	Baltimore,	39 17·8	76 36·6	" 1840	71 33·9
7	Washington,[4]	38 53·1	77 00·2	Sep. 1841	71 15·9
8	Harrisburg,	40 16	76 53	July 1840	72 20·5
9	Duncan's Island,	40 25	77 01	" 1840	72 35·0
10	Near Mercersburg,	39 47	77 56	Aug. 1840	71 47·3
	Mean,	39 57·1	76 16·8	1841·0	72 04·4

[3] The dip is the mean from groups of December 1840, October 1841, and August 1843.

[4] This station has been added to the discussion, as we have observations in 1840 and 1841; see *Appendix No.* 26, *Coast Survey Report* of 1858. Mean dip from several observers in 1841·0, 71° 18′·3, and in 1842·5, 71° 13′·5. Mean, 71° 15′·9 in 1841·8.

No.	Group II.	Latitude.	Longitude.	Date.	Observed dip.
		° ′	° ′		° ′
1	Armagh,	40 29	79 04	Aug. 1840	72 18·7
2	Frostburgh,	39 41	78 56	" "	71 31·3
3	Near Brownsville,	39 59·5	79 47·8	" "	71 53·5
4	Near Pittsburg,	40 28	79 59 5	" "	72 32·1
5	Economy,	40 37	80 16	" "	72 35·0
6	Wheeling,	40 08	80 42	" "	72 08·9
7	Steubenville,	40 25	80 39	" "	72 32·8
	Mean,	40 15 4	79 54·9	1840·6	72 13·2

No.	Group III.	Latitude.	Longitude.	Date.	Observed dip.
		° ′	° ′		° ′
1	Warren,	41 17	80 50	Aug. 1841	72 59·9
2	Mercer,	41 13 8	80 16	" "	72 57·2
3	Ashtabula Landing,	41 54	80 47	" "	73 23·5
4	Erie,	42 07·5	80 06	" "	73 46·6
5	Dunkirk,	42 29·3	79 23	" "	74 17·2
6	Ellicottsville,	42 18·1	78 44	" "	74 17·8
7	Berlin's Tavern,	41 16	79 36	" "	72 52·8
	Mean,	41 48·0	79 57·4	1841·6	73 30·7

No.	Group IV.	Latitude.	Longitude.	Date.	Observed dip.
		° ′	° ′		° ′
1	Curwinsville,	40 57·7	78 36	Aug. 1841	72 49·7
2	Belvidere,	42 13	78 06	" "	74 09·5
3	Bath,	42 20·8	77 21	" "	74 27·5
4	Owego,	42 08	76 17	" "	74 13·9
5	Silver Lake,	41 56·6	76 02	" "	73 41·5
6	Wilkesbarre,	41 14	75 58	July "	73 10·0
7	Williamsport,	41 14·0	77 02	" "	72 54·4
8	Bellefonte,	40 55	77 49	" "	72 42·3
9	Lewistown,	40 35	77 36	" 1840	72 30·0
10	Huntingdon,	40 30·5	78 02	" "	72 17·8
	Mean,	41 24.5	77 16·9	1841·4	73 17·7

No	Group V.	Latitude.	Longitude.	Date.	Observed dip.
		° ′	° ′		° ′
1	Niagara Falls,	43 04	79 05	Aug. 1843	74 51·0
2	Toronto Ob.,	43 39·5	79 21·5	" "	75 11·4
3	Rochester,	43 07	77 39	" "	74 43·5
4	Geneva,	42 53	77 02	July "	74 33·2
5	Syracuse,	43 03	76 09·3	" "	74 51·2
6	Oswego,	43 26	76 35	Aug. "	75 07·1
	Mean,	43 12.1	77 38·6	1843·6	74 52·9

No.	Group VI.	Latitude.	Longitude.	Date.	Observed dip.
		° ′	° ′		° ′
1	Utica,	43 05	75 14	July 1843	74 50·3
2	Schenectady,	42 48	73 57	" "	74 54·8
3	Troy,	42 43·7	73 40·7	Aug. "	74 47·9
4	West Point,	41 23·4	73 57 0	July "	73 12·2
5	New York,[1]	40 46·1	73 56·3	Dec. 1841	72 39·6
6	Milford,	41 19	74 51·5	Aug. "	73 47·6
7	Bushkill,	41 07	75 02	" "	73 31·4
8	Princeton,	40 20·7	74 39·6	July 1843	72 38·3
	Mean,	41 41·6	74 24·8	1842·9	73 47·8

[1] See *Appendix No.* 32, *Coast Survey Report* of 1856. This station was added, owing to the numerous observations taken in this locality (at Lunatic Asylum). Dip in 1841·3, 71° 41′·0; in 1842·5, 72° 38′·3.

Recapitulation.

No.	Group.	Latitude.	Longitude.	Date.	Observed dip.
		° ′	° ′		° ′
10	I.	39 57·1	76 16·8	1841·0	72 04·4
7	II.	40 15·4	79 54·9	1840·6	72 13·2
7	III.	41 48·0	79 57·4	1841·6	73 30·7
10	IV.	41 24·5	77 16·9	1841·4	73 17·7
6	V.	43 12·1	77 38·6	1843·6	74 52·9
8	VI.	41 41·6	74 24·8	1842·9	73 47·8
	Mean,	41 23·1	77 34·9	1841·85 (November)	73 17·8

(Number of observations = 48.)

By comparing the differences in latitude and corresponding differences in dip, for each place, with the mean values of the group, their general accordance was ascertained. None of the differences was large enough to require an exclusion from the series. It need hardly be remarked, that a slight consideration shows that the dip depends almost exclusively upon the latitude, and the longitude factors will, therefore, necessarily be very small.

Method of discussion.—The interpolation formula, proposed by the Rev. H. Lloyd in 1838 (see the *8th Report of the British Association*, vii, 91), will be used here in a slightly altered form, to allow for the convergence of the meridians.

Let I = resulting dip or inclination.

I_0 = assumed dip for the epoch adopted (1842·0) and the mean latitude and longitude, i its correction.

$d\,L$ = difference of latitude, $d\,M$ = difference of longitude.

x, y, z, p, q, as well as i, are to be determined by application of the method of least squares, from the observations themselves.

$$I = I_0 + i + x dL + y dM \cos L + z dL dM \cos L + p dL^2 + q dM^2 \cos^2 L.$$

Correction to epoch.—The mean epoch of the six groups is November 1841, for which we can substitute without material loss of accuracy January 1842 (or 1842·0). Comparing the observations made by Assistant Chas. A. Schott in July and August 1862, with the corresponding observations about the epoch 1842, we have the following table of differences of results for an interval of nearly 20 years:

Locality.	Date.	Dip.	Date.	Dip.	Av. Annual Increase.
		° ′		° ′	′
Washington,	Sept. 1841	71 15·9	Aug. 1862	71 19·0	+0·15
Harrisburg,	July 1840	72 20 5	July "	72 31·6	+0·50
Near Brownsville,	Aug. "	71 53·5	" "	71 56·9	+0·15
Erie,	" 1841	73 46·6	Aug. "	73 52·2	+0·27
Bath,	" "	74 27·5	" "	74 26·2	−0·06
Williamsport,	July "	72 54·4	" "	72 51·0	−0·16
Philadelphia,	Feb. 1842	71 57·1	" "	72 05·8	+0·43
				Mean,	+0·18

Mean total change in 21 years = 3′·8.

The increase in the dip is, therefore, very slight, and if we consider that, according to Mr. Schott's investigation (*Appendix, No.* 32, *Coast Survey for* 1856), the dip near the Atlantic coast about the years 1841–1844 was at its minimum value and hence could not have changed sensibly for several years—we can, without sacrificing anything in the accuracy of our reduction, use our results as if all belonging to the mean epoch 1842·0. No reduction to epoch has therefore been applied. It is probable that the present annual increase amounts to about 1′. At Toronto, between 1844 and 1855 (see *Toronto Observations*, vol. iii), the annual increase was 0′·8. In the formula of interpolation, I retain the factor cos L, thus making it comparable with similar expressions for other localities where the introduction of cos L may be more important.

The value of the magnetic survey of Pennsylvania is increased from the fact that the isoclinal lines are presented for an epoch at which the dip was probably near its minimum value.

The conditional equations are of the form

$$0=I_0-I+i+xd\mathrm{L}+yd\mathrm{M}\cos\mathrm{L}+zd\mathrm{L}d\mathrm{M}\cos\mathrm{L}+pd\mathrm{L}^2+qd\mathrm{M}^2\cos^2\mathrm{L}.$$

Next, nine groups of five or six observations in each, were formed, arranged in regard to the geographical position and area with as much regularity as the nature of the case admits of.

Recapitulation of mean values of groups.

Group.	Latitude.	Longitude.	Dip.
	°	°	°
I.	40·40	75·02	72·47
II.	39·56	76 85	71·73
III.	40·22	80·06	72·20
IV.	41·56	80·32	73·20
V.	40·65	78 02	72·54
VI.	42·75	78·93	74 56
VII.	41·53	76·07	73·50
VIII.	42·96	76·95	74·74
IX.	42·26	74·33	74·31
Mean,	41·32	77 39	73·25

The trial of an equation of the form,

$$I=I_0+i+xd\mathrm{L}+yd\mathrm{M}\cos\mathrm{L}+zd\mathrm{L}d\mathrm{M}\cos\mathrm{L};$$

and of the form,

$$I=I_0+i+xd\mathrm{L}+yd\mathrm{M}\cos\mathrm{L}+qd\mathrm{M}^2\cos^2\mathrm{L},$$

showed that the extent of the survey is not sufficiently great to admit of the determination of curvature of the isoclinal lines, and finally the following expression was adopted:

$$I=73^\circ\cdot25+0\cdot912\,d\mathrm{L}-0\cdot069\,d\mathrm{M}\cos\mathrm{L}.$$

This equation represents the observations as follows:

Group.	Observed Dip.	Computed Dip.	Diff. observed − computed.
	°	°	°
I.	72·47	72·54	−0·07
II.	71·73	71·68	+0·05
III.	72·20	72·11	+0·09
IV.	73·20	73·31	−0·11
V.	72 54	72·61	−0·07
VI.	74·56	74·47	+0·09
VII.	73·50	73·51	−0·01
VIII.	74·74	74·76	−0·02
IX.	74·31	74·26	+0·05

The isoclinal lines of 71°, 72° 73°, 74° and 75° pass through the following positions:

71°,	Long.	77° 00′		
	Lat.	38 49		
72°,	Long.	75 00	78° 00′	81° 00′
	Lat.	39 49	39 59	40 10
73°,	Long.	74 00	78 00	81 00
	Lat.	40 50	41 05	41 15
74°,	Long.	74 00	78 00	81 00
	Lat.	41 57	42 11	42 22
75°,	Long.	75 00	77 00	79 00
	Lat.	43 07	43 13	43 20

These lines have been finally adopted.

Comparison of the observed and computed dip.

	Observed Dip.	Computed Dip.	Diff. observed − computed.
Group I.	°	°	°
New York,	72·66	72·93	−0·27
Easton,*	72·65	72·80	−0·15
Princeton,*	72·64	72·51	+0·13
Doylestown,*	72·39	72·44	−0·05
Reading,*	72·54	72·42	+0·12
Philadelphia,	71·95	72·14	−0·19
Group II.			
Frenchtown,*	71·67	71·75	−0·08
Baltimore,	71·57	71·45	+0·12
Washington,	71·26	71·06	+0·20
Harrisburg,	72·34	72·32	+0·02
Near Mercersburg,	71 79	71·88	−0·09
Group III.			
Frostburgh,*	71·52	71·67	−0·15
Near Brownsville,	71·89	71·91	−0·02
Wheeling,*	72·15	71·99	+0·16
Steubenville,*	72·56	72·26	+0·30
Near Pittsburg,	72·53	72·24	+0·29
Economy,*	72·58	74·46	+0·12

* All stations where the dip has been found indirectly only, by means of the Lloyd needles, are marked with an asterisk,—27 in number. Total number of stations 48.

Table continued.

	Observed Dip.	Computed Dip.	Diff. observed − computed.
Group IV.			
Berlin Tavern,*	72·88	73·09	−0·21
Mercer,	72·95	73·02	−0·07
Warren,*	73·00	73·03	−0·03
Ashtabula,*	73·39	73 60	−0·21
Erie,	73·78	73·85	−0·07
Group V.			
Duncan's Island,*	72·58	72·45	+0·13
Lewistown,*	72·50	72·57	−0·07
Huntingdon,	72·30	72·48	−0·18
Armagh,*	72·31	72·40	−0·09
Bellefonte,*	72·70	72·86	−0·16
Curwinsville,	72·83	72·86	−0·03
Group VI.			
Belvidere,*	74·16	74·03	+0·13
Ellicottsville,	74·30	74·05	+0·25
Dunkirk,*	74·29	74·21	+0·08
Niagara Falls,*	74·85	74·76	+0·09
Toronto,	75·19	75·28	−0·09
Group VII.			
Bushkill,*	73·52	73·19	+0·33
Williamsport,	72·91	73·19	−0·28
Wilkesbarre,*	73·17	73·24	−0·07
Silver Lake,	73·69	73·88	−0·19
Owego,*	74.23	74·05	+0·18
Group VIII.			
Bath,	74·46	74·19	+0·27
Rochester,*	74·72	74·87	−0·15
Geneva,	74 55	74·69	−0·14
Syracuse,	74 85	74·89	−0·04
Oswego,*	75·12	75·21	−0·09
Group IX.			
West Point,*	73·20	73·49	−0·29
Milford,	73·79	73·38	+0·41[†]
Utica,*	74·84	74·96	−0·12
Schenectady,	74·91	74·77	+0·14
Troy,*	74·80	74·72	+0·08

The probable error of any single observation is $\pm 0°·12 = \pm 7'·2$; the probable error of any observation with the regular dip needles and the Lloyd needles combined is $\pm 0°·13$; with the latter needles alone, $\pm 0°·11$. This shows that the irregularities in the observed dip are due to local attractions rather than to imperfections in the needles employed. It is proper therefore to assign equal weights to results by the direct and indirect method of observing.

If we apply Peirce's criterion for the rejection of observations differing too much from the regular value indicated by all other observations, we find the limit of rejection to be $\pm 0°·46$ or $\pm 28'$; the maximum difference in the preceding table is $25'$, hence no observation is excluded.

[†] Maximum difference = 25′.

Gen. Sabine's resulting isoclinal lines in his seventh contribution to terrestrial magnetism (*Phil. Trans. Roy. Soc.*, Part III, 1846, p. 237) refer to an average period between 1840 and 1842, and correspond in their position very closely to those now presented;—they are deduced from independent data.

Distribution of the Magnetic Horizontal Intensity, and construction of isodynamic lines for 1842.

If we group the observed intensities in the same manner as the dip, the mean epoch 1842·0 may likewise be assumed, and all observed intensities be reduced to that date.

Correction to epoch.—We have the following direct comparisons with Mr. Schott's observation of 1862.

Locality.	Date.	X.	Date.	X_1.	$X-X_1$.	Annual decrease.
Washington,[8]	Jan. 1843	4·320	Aug. 1862	4·255	0·065	0·0033
Harrisburg,	July 1840	4·078	July "	4·012	0·066	0·0030
Near Brownsville,	Aug. "	4·207	" "	4·138	0·069	0·0031
Erie,	" 1841	3·792	Aug. "	3·728	0·064	0·0030
Bath,	" 1841	3·677	" "	3·639	0·038	0·0018
Williamsport,	July 1841	3·983	" "	3·924	0·059	0·0028
Philadelphia,[9]	Jan. 1842	4·166	" "	4·088	0·078	0·0039
					Mean,	0·0030

The average annual decrease in the value of X, between 1840 and 1862, is, therefore, 0·0030, or when expressed in parts of X, equal to 0·00076. This result agrees tolerably well with that deduced by Mr. Schott in the *Coast Survey Report* of 1861, where 0·00110 was found.

Supposing the dip to increase at the rate of 1′ a year, and the total intensity to remain constant, the corresponding decrease of the horizontal intensity would amount to nearly the quantity found above, and we can not, therefore, as yet decide whether the total intensity remains stationary or is slightly changing. At Toronto (see *Toronto Observations*, vol. iii), the annual decrease of X between 1845 and 1852 inclusive was 0·0037 (in absolute measure), or 0·00105 when expressed in parts of X.

Formation of groups for the analytical expression of the distribution of the magnetic horizontal force, referred to the epoch 1842·0.

At stations marked thus *, the horizontal force was determined by vibrations; at those not marked, by Lloyd's statical method.

[8] From *Coast Survey Report* of 1861 (yet in manuscript),
in 1842·5, Capt. Lefroy, X=4·347
" 1843·5, Dr. Locke, =4·292 (mean of three results).
Mean in 1843·0, =4·320

[9] In July and November, 1840, X=4·160
" " " " 1841, 4·166 } Mean, 4·166, for 1842·0
" July, 1843, 4·172

	Date.	X.	Correction to epoch.	$X_{1842\cdot0}$
Group I.				
Philadelphia,*	1842·0	4·166	0·000	4·166
Doylestown,	1841·6	4·189	−0·001	4·188
Easton,	1841·6	4·121	−0·001	4·120
Reading,	1840·6	4·000	−0·004	3·996
Frenchtown,	1840·6	4·312	−0·004	4·308
Baltimore,*	1840·6	4·265	−0·004	4·261
Washington,	1843·0	4·320	+0·003	4·323
Harrisburg,	1840·6	4·078	−0·004	4·074
Duncan's Island,	1840·6	3·963	−0·004	3·959
Near Mercersburg,*	1840·6	4·188	−0·004	4·184
			Mean,	4·158
Group II.				
Armagh,	1840·6	4·038	−0·004	4·034
Frostburg,	1840·6	4·298	−0·004	4·294
Near Brownsville,*	1840·6	4·207	−0·004	4·203
Near Pittsburg,*	1840·6	4·049	−0·004	4·045
Economy,	1840·6	4·008	−0·004	4·004
Wheeling,	1840·6	4·053	−0·004	4·049
Steubenville,	1840.6	3·947	−0·004	3·943
			Mean,	4·082
Group III.				
Warren,	1841·6	3·978	−0·001	3·977
Mercer,*	1841·6	4·000	−0·001	3·999
Ashtabula,	1841·6	3·838	−0·001	3·837
Erie,*	1841·6	3·792	−0·001	3·791
Dunkirk,	1841·6	3·621	−0·001	3·620
Ellicottsville,*	1841·6	3·726	−0·001	3·725
Berlin's Tavern,	1841·6	4·026	−0·001	4·025
			Mean,	3·853
Group IV.				
Curwinsville,*	1841·6	3·999	−0·001	3·998
Belvidere,	1841·6	3·669	−0·001	3·668
Bath,*	1841·6	3·677	−0·001	3·676
Owego,	1841·6	3·614	−0·001	3·613
Silver Lake,*	1841·7	3·782	−0·001	3·781
Wilkesbarre,	1841·6	3·961	−0·001	3·960
Williamsport,*	1841·6	3·983	−0·001	3·982
Bellefonte,	1841·6	4·069	−0·001	4·068
Lewistown,	1840·6	3·984	−0·004	3·980
Huntingdon,*	1840·6	4·109	−0·004	4·105
			Mean,	3·883
Group V.				
Niagara Falls,*	1843·6	3·565	+0·005	3·570
Toronto Ob.,*	1843·6	3·537	+0·005	3·542
Rochester,	1843·6	3·560	+0·005	3·565
Geneva,*	1843·7	3·635	+0·005	3·640
Syracuse,*	1843·6	3·556	+0·005	3·561
Oswego,	1843·6	3·467	+0·005	3·472
			Mean,	3·558

Table continued.

	Date.	X.	Correction to epoch.	$X_{1842\cdot0}$
Group VI.				
Utica,	1843·6	3·541	+0·005	3·546
Schenectady,*	1843·6	3·502	+0·005	3·507
Troy,	1843·6	3·575	+0·005	3·580
West Point,	1843·6	4·033	+0·005	4·038
New York,[10]	1841·9	4·014	0·000	4·014
Milford,*	1841·7	3·769	−0·001	3·768
Bushkill,	1841·7	3·866	−0·001	3·865
Princeton,	1843·5	4·222	+0·005	4·227
			Mean,	3·818

Recapitulation.

Group.	No.	Latitude.	Longitude.	$X_{1842\cdot0}$.
		° ′	° ′	
I.	10	39 57·1	76 16·8	4·158
II.	7	40 15·4	79 54·9	4·082
III.	7	41 48·0	79 57·4	3·853
IV.	10	41 24·5	77 16·9	3·883
V.	6	43 12·1	77 38·6	3·558
VI.	8	41 41·6	74 24·8	3·818
	Mean,	41 23·1	77 34·9	3·892

Let X= resulting horizontal force.

X_0= assumed mean horizontal force for 1842·0 at the mean latitude and mean longitude, χ its correction.

dL= difference of latitude, dM= difference of longitude.

x, y, z, p, q and χ to be determined from the observations.

$$X=X_0+\chi+x\,dL+y\,dM\cos L+z\,dL\,dM\cos L+p\,dL^2+q\,dM^2\cos^2 L.$$

Forming the conditional and normal equations, we find the expression

$$X=3\cdot890-0\cdot1787\,dL+0\cdot0085\,dM\cos L+0\cdot0161\,dL\,dM\cos L-0\cdot0017\,dL^2+0\cdot0027\,dM^2\cos^2 L.$$

where

$$dL=\text{lat.}-41°\cdot38$$
$$dM=\text{long.}-77\cdot58.$$

This formula is applied for determining the relative weights of the observations from vibrations and by deflections of the dipping needle; for this purpose, the horizontal force was computed by the formula, and the results compared with observation. From the differences, we find the probable error of an observation (and local irregularity) $=\pm0\cdot036$ for the bar and cylinder vibrations, and $\pm0\cdot062$ for the Lloyd needle deflections and dip; the relative weights therefore become 754 for the former and 257 for the latter, or nearly as 3 to 1.

These weights have been adopted.

[10] At New York we have: 1841·5, Dr. Locke, 4·015; 1842·7, Dr. Locke, 4·008; 1842·7, Capt. Lefroy, 4·010; mean, 4·014, for 1841·9.

Nine groups of five or six observations in each, with weights, were then formed.

Recapitulation of mean (weighted) values of groups.

Group.	Latitude.	Longitude.	X.
	°	°	
I.	40·39	74·83	4·107
II.	39·68	77·01	4·199
III.	40·22	79·99	4·103
IV.	41·61	80·26	3·912
V.	40·68	78·14	4·035
VI.	42·89	79·00	3·618
VII.	41·56	76·27	3·858
VIII.	42·85	77·17	3·606
IX.	42·17	74·37	3·665
Mean,	41·34	77·45	3·900

$$X=X_0+\chi+xdL+ydM\cos L+xdLdM\cos L+pdL^2+qdM^2\cos^2 L.$$

$$dL=\text{lat.}-41°{\cdot}34$$

$$dM=\text{long.}-77\cdot45.$$

Forming the conditional and normal equations we deduce:

$$X=3{\cdot}920-0{\cdot}1936\,dL+0{\cdot}0146\,dM\cos L+0{\cdot}0203\,dLdM\cos L$$
$$-0{\cdot}01587\,dL^2-0{\cdot}0005\,dM^2\cos^2 L.$$

It is, however, preferable to shorten the formula and use instead the following:

$$X=3{\cdot}900-0{\cdot}1934\,dL+0{\cdot}0134\,dM\cos L+0{\cdot}02\,dLdM\cos L.$$

Comparison of observed and computed values.

Group.	X observed.	X computed.	Observed −computed.
I.	4·107	4·095	+0·012
II.	4·199	4·227	−0·028
III.	4·103	4·100	+0·003
IV.	3·912	3·887	+0·025
V.	4·035	4·028	+0·007
VI.	3·618	3·651	−0·033
VII.	3·858	3·842	+0·016
VIII.	3·606	3·599	+0·007
IX.	3·665	3·670	−0·005

The next and last hypothesis,

$$X=3{\cdot}900-0{\cdot}1934\,dL+0{\cdot}0134\,dM\cos L,$$

in which the isodynamic lines are treated as straight lines, presents perhaps the best and most simple expression of the *irregular* distribution of the horizontal force.

These lines are nearly parallel with the dip lines.

Comparison of observed and computed values on this hypothesis.

Group.	X observed.	X computed.	Observed −computed.
I.	4·107	4·057	+0·050
II.	4·199	4·216	−0·017
III.	4·103	4·143	−0·040
IV.	3·912	3·876	+0·036
V.	4·035	4·035	0·000
VI.	3·618	3·616	+0·002
VII.	3·858	3·846	+0·012
VIII.	3·606	3·605	+0·001
IX.	3·665	3·708	−0·043

The difference between the lines of this and the previous hypothesis shows the large amount of local irregularity.

The lines of this hypothesis pass through the following positions:

4·2	Long. 81°·0	Long. 77°·5	Long. 74°·0
	Lat. 39 58′	Lat. 39 47′	Lat. 39 36′
4·0	Long. 81°·0	Long. 77°·5	Long. 74°·0
	Lat. 41 01′	Lat. 40 49′	Lat. 40 39′
3·8	Long. 81°·0	Long. 77°·5	Long. 74°·0
	Lat. 42 02′	Lat. 41 51′	Lat. 41 41′
3·6	Long. 81°·0	Long. 77°·5	Long. 74°·0
	Lat. 43 04′	Lat. 42 53′	Lat. 42 43′

The observed and computed values of X by the previous and last hypothesis compare as follows:

Station.	X observed.	X by previous hypothesis.	Δ	X by last hypothesis.	Δ
Philadelphia,*	4·17	4·19	−0·02	4·14	+0·03
Doylestown,	4·19	4 11	+0·08	4·08	+0·11
Easton,	4·12	4·02	+0·10	4·00	+0·12
Reading,	4·00	4·10	−0·10	4·08	−0·08
Frenchtown,	4·31	4·27	+0·04	4·22	+0·09
Baltimore,*	4·26	4 32	−0·06	4·29	−0·03
Washington,	4·32	4·38	−0·06	4·37	−0·05
Harrisburg,*	4·07	4·11	−0·04	4·10	−0·03
Duncan's Island,	3·96	4·08	−0·12	4·07	−0·11
Near Mercersburg,*	4·18	4·21	−0 03	4·20	−0 02
Armagh,	4 03	4 06	−0·03	4 08	−0·05
Frostburgh,	4·29	4·20	+0 09	4·24	+0 05
Near Brownsville,*	4·20	4·14	+0·06	4·19	+0·01
Near Pittsburg,*	4·05	4·06	−0·01	4·09	−0·04
Economy,	4·00	4·04	−0·04	4·07	−0·07
Wheeling,	4·05	4·11	−0·06	4·17	−0·12
Steubenville,	3·94	4 07	−0·13	4·11	−0·17
Warren,	3 98	3 94	+0·04	3·95	+0·03
Mercer,*	4·00	3·94	+0·06	3·95	+0·05
Ashtabula,	3·84	3·80	+0·04	3·83	+0·01
Erie,*	3·79	3 81	−0·02	3·77	+0·02
Dunkirk,	3·62	3·70	−0·08	3·70	−0·08
Ellicottsville,*	3 72	3·75	−0·03	3·73	−0·01
Berlin's Tavern,	4·02	3·93	+0·09	3·94	+0·08
Curwinsville,*	4·00	3·98	+0 02	3·99	+0·01
Belvidere,	3·67	3·75	−0 08	3·74	−0 07
Bath,*	3·68	3·70	−0·02	3·70	−0 02
Owego,	3 61	3 73	−0·12	3·74	−0·13
Silver Lake,*	3·78	3·76	+0·02	3·77	+0·01
Wilkesbarre,	3·96	3·93	+0·03	3·91	+0·05
Williamsport,*	3·98	3·92	+0·06	3·92	+0·06
Bellefonte,	4·07	3 99	+0·08	3 99	+0·08
Lewistown,	3·98	4·05	−0·07	4·05	−0·07
Huntingdon,*	4·10	4·06	+0·04	4·07	+0·03
Niagara Falls,*	3·57	3·62	−0 05	3 58	−0 01
Toronto,*	3·54	3 53	+0·01	3·47	+0·07
Rochester,	3·56	3·57	−0·01	3·56	0·00
Geneva,*	3·64	3·59	+0·05	3·60	+0·04
Syracuse,*	3 56	3·53	+0·03	3·56	0·00
Oswego,	3·47	3·46	+0·01	3·49	−0·02

Table continued.

Stations.	X observed.	X by previous hypothesis.	Δ	X by last hypothesis.	Δ
Utica,	3·55	3·49	+0·06	3·54	+0·01
Schenectady,*	3·51	3·51	0·00	3·58	−0·07
Troy,	3·58	3·52	−0·06	3·59	−0·01
West Point,	4·04	3·85	+0·19	3·86	+0·18
New York,*	4·01	4·01	0·00	3·97	+0·04
Milford,*	3·77	3·88	−0·11	3·88	−0·11
Bushkill,	3·86	3·92	−0·06	3·92	−0·06
Princeton,	4·23	4·07	+0·16	4·06	+0·17

For the last hypothesis (straight lines), we find the probable error of an observation and local irregularity from the bar and cylinder vibrations $\pm 0{\cdot}029$, and from the Lloyd needle deflection and dip $\pm 0{\cdot}062$. For the previous hypothesis, these quantities are respectively $\pm 0{\cdot}030$ and $\pm 0{\cdot}059$, showing but little gain in the representation of the observations by the additional term $d\mathrm{L}d\mathrm{M}\cos \mathrm{L}$.

For the general representation, the probable errors are $\pm 0{\cdot}050$ and $\pm 0{\cdot}051$.

Representation of the total force.

From the expressions

$$\mathrm{X}=3{\cdot}900-0{\cdot}1934\ d\mathrm{L}+0{\cdot}0134\ d\mathrm{M}\cos \mathrm{L},$$
$$\mathrm{I}=73°{\cdot}25+0{\cdot}912\ d\mathrm{L}-0{\cdot}0690\ d\mathrm{M}\cos \mathrm{L},$$

we have to deduce the total force $\varphi=\mathrm{X}\sec \mathrm{I}$.

In the expression for X,

$$d\mathrm{L}=\text{lat.}-41°{\cdot}34 \text{ and } d\mathrm{M}=\text{long.}-77°{\cdot}45,$$

in the expression for I,

$$d\mathrm{L}=\text{lat.}-41°{\cdot}32 \text{ and } d\mathrm{M}=\text{long.}-77°{\cdot}39.$$

We have in

Long. 81°·00	X= 4·200	}	$\varphi=13{\cdot}47$
Lat. 39 ·97	I =71°·828	}	
Long. 77°·50	X= 3·600	}	$\varphi=13{\cdot}62$
Lat. 42 ·89	I =74°·676	}	
Long. 74°·00	X= 4·200	}	$\varphi=13{\cdot}49.$
Lat. 39 ·60	I =71°·861	}	

Assuming in the expression for the total force,

$$\varphi=\varphi_0+f+x d\mathrm{L}+y d\mathrm{M}\cos \mathrm{L},$$

$d\mathrm{L}$ and $d\mathrm{M}$ as in the expression for X, we find:

$$\varphi=13{\cdot}55+0{\cdot}0451\ d\mathrm{L}-0{\cdot}00682\ d\mathrm{M}\cos \mathrm{L}.$$

The lines of equal total force of 13·45, 13·5, 13·55 and 13·6 pass through the following positions:

13·45	Long. 81°	Long. 77°·5	
	Lat. 39 31′	Lat. 39 07′	
13·50	Long. 81°	Long. 77°·5	Long. 74°
	Lat. 40 37′	Lat. 40 13′	Lat. 39 49′
13·55	Long. 81°	Long. 77°·5	Long. 74°
	Lat. 41 43′	Lat. 41 19′	Lat. 40 55′
13·60	Long. 81°	Long. 77°·5	Long. 74°
	Lat. 42 49′	Lat. 42 25′	Lat. 42 01′

The observed and computed values of φ, at the stations where the bar and cylinder were employed, compare as follows:

Station.	φ observed.	φ computed.	Observed − computed.
Philadelphia,	13·45	13·50	−0·05
Harrisburg,	13·44	13·50	−0·06
Huntingdon,	13·51	13·51	0·00
Homewood,	13·49	13·50	−0·01
Johnson's Tavern,	13·54	13·48	+0·06
Irwin's Mill,	13·40	13·48	−0 08
Baltimore,	13·49	13·46	+0·03
Williamsport,	13·55	13·55	0 00
Curwinsville,	13·55	13·53	+0·02
Mercer,	13·64	13·53	+0·11
Erie,	13·57	13·57	0 00
Ellicottsville,	13·77	13·59	+0·18
Bath,	13·72	13 60	+0·12
Silver Lake,	13·47	13·58	−0·11
Milford,	13·50	13 56	−0·06
Schenectady,	13·45	13·63	−0·18
Syracuse,	13·61	13 63	−0·02
Geneva,	13·63	13·62	+0 01
Niagara Falls,	13·64	13·62	+0·02
Toronto,	13 84	13·65	+0·19

The probable error of any representation is ±0·066.

ART. XXXVIII.—*On some questions concerning the Coal Formations of North America;* by LEO LESQUEREUX. (Continued from vol. xxxiii, p. 216.

Concluding Remarks on the Fossil Ferns.

THE examination of the fossil ferns of the coal, as far as it has passed under review in the former papers,[1] would apparently authorize the three following conclusions.

1st. That the family of the Ferns was represented, at the coal epoch, by species whose forms are easily referred to very few typical forms. For, if we consider only the figure of the leaves, viz., their contour and nervation, the only part generally preserved in the shales of the Coal Measures, all the species may

[1] Vol. xxxii, p. 193, Sept., 1861, and vol. xxxiii, p. 206, March, 1862.

be comprised in the three sections formerly examined: the *Neuropterideæ*, the *Pecopterideæ* and the *Sphenopterideæ*.

2d. That, from the scarcity of fructified specimens of fossil ferns in the Coal Measures, it would be supposed that most of the species were without fruit. If not, how can we account for the total destruction of the sporanges, either borne on peculiar stems, or attached to the lower surface of the leaves, as we find them in the species of our time?

3d. That the scarcity of large stems that have been or might be referred to ferns would lead us to suppose that, during the formation of the coal, the fern trees were of rare occurrence, at least when compared with the great number of ferns, which, in opposition to arborescent species, can be called herbaceous or shrubby.

These three questions must be considered separately.

1st. If it is certain that characters taken from the form of the leaves and from their nervation are sufficient for a kind of general classification, applicable to the stratification of the Coal Measures, it is true also that this classification fails to give us a clear insight into the true relation and the affinity of our fossil species. To be exact and scientific, an analysis of the ferns must take into account the form and the position of the fructifications; and when these are absent or undiscernable, as is generally the case with the specimens found in the Coal Measures, we are not authorized to believe that all the species, referable by their nervation and the form of the leaves to a common type, are equally related to it, by more essential but unknown characters. Indeed, though the attempt at a classification of the fossil ferns of the coal, from their fructifications, has been till now an abortive effort, the little we know of these fructifications shows a far greater diversity of typical and generic forms than are indicated by the leaves and their nervation. The fruiting leaves of the *Neuropterideæ* are known, from European specimens, only for the genus *Odontopteris*, which, as Mr. Brongniart has remarked, do not bear any relation to ferns of our time. Species of this genus have their spores enclosed in a kind of bladdery sporange, or between the surfaces of the leaflets, which, thus inflated, wrinkled and without any trace of nerves, entirely lose their original shape. American specimens of this species perfectly agree with the beautiful figures that Mr. Gœppert has given of it in his *Genera*. The peculiar shape of the fructifications of this genus is still more remarkable on the fruiting specimens of *Odontopteris Britannica* Gutb., a species which has not yet been found in our American Coal Measures. Its sporanges, placed along a strong rachis and on both sides of it, have the form of an oval-pointed nutlet and rather resemble a raceme of fruit, or rather a branch bearing buds of flowers, of a dicotyledonous

species. Fine specimens of these supposed *Antholites* have been published by Lindley and Hutton, from the Coal Measures of England, and by Dr. Newberry, from our own coal fields. I have found also some small specimens of these peculiar remains at Pomeroy, Ohio, and at Port Carbon, Penn. All these, either naked or bracteated nutlets, appear to be only branches of fruiting stems of some ferns of an unknown type.

The fructifications of the genus *Neuropteris*, which, by the form of the leaves and the nervation, is closely allied to *Odontopteris*, appear to have a far different character. These fructifications, I think, are not known from European specimens; but we have a few from the American Coal Measures, which can be reliably considered as the remains of fruiting parts of some species of this genus. Two of them represent reduced forms of the upper part of a penna of *Neuropteris hirsuta* Lsqx. The leaflets are longer and narrower; the rachis and the medial nerve are flat and broad, and look rather like the branches and divisions of a panicle bearing sporanges. The veins are prominent, granulate, just as if a series of small fruit dots connected together had been placed either along them, or in the narrow space that separates them. If this appearance is real, these fructifications would bear a likeness of position to those of the *Danaeæ* of our time. But not of direction, indeed; for, in this species of our Coal Measures, the nervules are arched and dichotomous or forking like those of a true *Neuropteris*. Another remarkable specimen, preserved in a pebble of carbonate of iron, from Morris Co., Illinois, represents also a branch of a species of fructified *Neuropteris*. In this, the short, ovate, slightly pointed leaflets, about one inch long, and deeply cordate at the base, are attached to the rachis by a short pedicel. They are slightly convex or inflated in the middle, with a narrow margin apparently reflexed, but at the same time flattened all around. The scarcely visible veins are distant and apparently forked once, or the surface, generally quite smooth, is marked by irregular undulate cross-wrinkles, somewhat resembling those of the fructifications of an *Odontopteris*. In this case, the spores appear to be placed in large flakes, covering, except a narrow border, the whole of the lower surface of the leaves, as is the case with the fruit-bearing leaves of some species of *Osmunda* of our time. Thus, in the same genus, there are apparently two far different types of fructifications.

A peculiar specimen of fruiting fern, belonging to the Cabinet of Amherst College, and labelled, Mansfield,? Mass., shows a pinnately divided frond or rather panicle, whose secondary rachis is pinnately subdivided into short branches, bearing numerous groups of fruit dots, placed four by four on each side of a common branchlet. They appear attached to it, each by a very slender pedicel; and, round as they are, with a depressed point

in the middle, they look, at first sight, like the fruit dots placed on both sides of the medial nerve of a *Pecopteris*, whose derma or foliaceous tissue has been entirely destroyed. As no trace of this tissue can be seen, as the pedicels do not resemble veins, but are curved in a peculiar way, and as the fruit dots are at some places scattered and not in regular order, this fossil raceme is more likely the fruit-bearing part of a species whose sterile frond is possibly known with other characters. If it is so, this species would have a relation to the genus *Aneimia* of the living ferns, and thus, it could not enter into any of the three general divisions mentioned above.

The same can be said of *Staphylopteris stellata* Lsqx., of the low coal of Arkansas.[2] The figured specimen represents a smooth, main stem or branch, pinnately divided into short, thick, horizontal branchlets, each bearing at its extremity a group of four or five oval sporanges, attached to a common receptacle. This species has no affinity whatever with any other fossil remains of ferns, found in the Coal Measures, and thus it is without a place in our general classification.

From a number of fruiting branches of still undescribed fossil ferns, I will only briefly describe another remarkable species recently found at Mason Creek, Morris Co., Ill., by Mr. Even, who has sent, from the same locality, many interesting specimens, beautifully preserved in pebbles of carbonate of iron.[3] The specimen shows the upper part of a pinnately divided frond. The divisions are short, (one inch) and comparatively broad, (one-sixth of an inch) linear, obtuse, spreading, decurring in preserving the same breadth on a slender rachis, which, thus broadly winged, looks rather like the primary nerve of a secondary pinna. The veins, emerging in a broad angle from this common rachis, are straight, pretty thick, ascending to the top of the divisions and pinnately branching. The distant simple veinlets, no more than three or four on each side, slightly arched, diverging in a broad angle, bear at their extremity a group of six oval sporanges, placed just on the borders of the divisions. These sporanges, united by their margins around a common receptacle, appear, by this disposition, like small stars with round lobes. Considering only the form of the leaves, this species should be

[2] Geological Report of the State Survey of Arkansas, ii, 309, pl. 2 fig. 2.

[3] A deposit of the same nature, a bank of clay iron ore with pebbles of carbonate of iron, is also found in Southern Ohio, northwest of Marietta. Nearly all the pebbles have as a matrix a piece of fern or of some other fossil plant. As the species are the same both in Illinois and in Ohio, I consider both these strata, from palæontological evidence, as having the same geological horizon. Their place, according to the same evidence, is at or near the level of Coal No. 4, just below the base of the Mahoning sandstone. The most abundant species are *Pecopteris unita* Brgt., *Neuropteris hirsuta* Lsqx., *Pecopteris arborescens* Brgt., *Pecopteris Miltoni* Brgt., *Hymenophyllites hirsuta* Lsqx., *Alethopteris Serlii* Brgt., *Asterophyllites*, *Sphenophyllum*, *Annularia* and *Neuropteris Loschii* Brgt.

placed in the genus *Alethopteris*. But it differs widely from it by its nervation and especially by its fructifications. These would bring this species near the genus *Asterocarpus* of Gœppert, or the *Heptocarpus* of Braun, to which it has no affinity whatever by the leaves and the nervation. From examples like this, which, though few in number, are nevertheless every day multiplied by new discoveries, we can admit, I think, for the coal epoch, a far greater diversity of typical forms than could be supposed at first sight and from superficial researches.

2. What is said above is already an answer to the second question concerning the scarcity of fruiting specimens of ferns in the Coal Measures. This scarcity, like the paucity of typical forms in the fossil ferns, is rather casual than real. By careful researches at some places, where the remains of a species are found in abundance, one may generally succeed in finding traces of fructifications. They are especially preserved on specimens found as matrix of iron agglomerations, which have not been exposed to maceration in water for too long a time. This of course confirms the validity of the conclusions arrived at by Prof. Lindley and Prof. Gœppert from their experiments on the action of the maceration in destroying or preserving the forms of some species of plants. Most of the species of ferns of our time, under a protracted and continual immersion, have preserved well enough the forms of their leaves with evident traces of their nervation; but they have lost their fructifications. The sporanges have been detached from their supports and destroyed.

It is moreover known that, nearly always, the fern leaves are attached to the shales by their lower surface. Thus, even when the fructifications are preserved, we cannot see them, or we have only an indistinct outline of their form, printed in relievo through the carbonized tissue of the leaves. This of course renders the study of the fossil fruiting ferns very difficult.

3. Is the small proportion of fossil remains of true arborescent ferns in the Coal Measures, compared with the great quantity of leaves and stalks or petioles of the same family, a proof that, contrary to the opinion generally admitted, the arborescent ferns were not a predominant character of the vegetation of the coal epoch? If we consider as remains of true arborescent ferns, only those whose bark is marked by large oval cicatrices, left at the base of the fronds, at the point of their parting from the main stem, in short those known under the family name of *Caulopterideæ* or *Protopterideæ*, it is certain that they are very scarce in the Coal Measures both of Europe and of America. In his *Genera*, Mr. Unger counts in the *Protopterideæ* of the coal ten species only, distributed in five genera. And from these species, five are considered by Brongniart, Lindley and other authors as pertaining to *Sigillaria* or *Lepidodendron*. In his

Tableau des genres, Brongniart enumerates only six species of *Caulopterideæ;* Geinitz in his *Wersteinerungen von Sachsen*, four species, three published by Brongniart as *Sigillaria*, and one, a *Megaphytum*, by Artes; and Gœppert, in his *Fossil Flora des Uebergangsgebirges*, has none. In my examination of the fossil plants of our Coal Measures, I have seen, from the roof shales of the coal, only three specimens of two different species found at Carbondale,[4] and one found in Illinois.[5] And from the sandstone of the Coal Measures, I have in my cabinet a single specimen from Ohio, and there is another of a different species in the Illinois State Cabinet.[6] A few others, like *Sigillaria Cistii* Brgt., from Wilkesbarre, Penn., *Sigillaria discoidea* Lsqx., from Summit Lehigh, Penn., *Megaphytum protuberans* Lsqx., *Megaphytum Wilburianum* Lsqx., and *Lepidodendron radicans* Lsqx., from Illinois; these three last figured and described in the Geological Report of that State, may still be referable to this group of plants.

If, on the contrary, we admit with most of the European authors, that the fossil trunks, generally comprised in the genus *Psaronius*, did belong to arborescent ferns, we have to come to quite a different conclusion, concerning the distribution of the vegetation of the ferns at the coal epoch; for these trunks are found in great abundance in some parts of the Coal Measures. But Prof. Brongniart, judging from their internal structure, considers *Psaronius* as a genus related to the *Lepidodendra* rather than to the *Protopteridæ* or ferns. As the *Psaronius* species have their stems generally encased in a thick coat of roots or rootlets, grown and petrified together, the surface of the stems and the cicatrices with which they were originally covered are scarcely to be seen. Nevertheless, among the great quantity of specimens which I have examined in Southern Ohio, I have found a few, the smallest in size, whose uncovered stems evidently bear the long oval scars, the external character of the arborescent ferns.

Now, admitting the species of *Psaronius* as true arborescent ferns, the question of their distribution in the Coal Measures and of the place and importance which they occupied in the vegetation of the coal epoch is still unsolved. Where did they come from, all these trunks of the same genus; all with the same peculiar structure; all horizontally broken in fragments varying from one inch to one foot in length, and thus scattered at some peculiar and isolated localities, where they appear as if they had been heaped by some wonderful and unaccountable agency? I do not know in our Coal Measures of another deposit of petrified trunks of fern trees except that of Shade river, Ohio. It begins at Athens and extends southward as far as Charleston, Va. At least, I have seen trunks of *Psaronius* scattered along

[4] Penn. Geol. Rept. p. 869, pl. 13, figs. 1 and 2.
[5] Ill. Geol. Rept. ined., pl. 13, fig. 1, under the name of *Caulopteris insignis.*
[6] *Caulopteris Worthenii*, sp. nov., Ill. Geol. Rept. ined., pl. 14, fig. 1.

the banks of the Great Kanawha from its mouth to Charleston. The geological horizon of the strata with which they are connected is not satisfactorily determined; though it is certain that their place is not far above the top of the Mahoning Sandstone. They are apparently imbedded in a kind of soft sandstone, which at Shade river is separated by a covered space of 10 feet from a bed of coal 10 inches thick, which I consider as the equivalent of Coal No. 5. I say apparently, because it is not certain that they were originally derived from this bed of soft sandstone or hard clay, exposed on the high water of Shade river, where they are seen in great quantity, heaped in all possible positions and directions, just as if they had been transported and deposited there by a strong eddy. Nevertheless, they do not bear any trace of erosion by water. The fracture is clean and often sharply marked all around their circumference. When they appear eroded, this erosion is evidently due to the process of maceration, at or before the time of petrification. As no remains of this genus are found in connection with the shales of the coal strata, I think that forests of these peculiar arborescent ferns did cover some dry, sandy places of the Coal Measures, in the vicinity of some hot springs perhaps, or under the influence of peculiar atmospheric action. There they may have lived around the marshes, and their prostrated stems have been petrified afterwards by a local influence. I believe that if we could satisfactorily explain the dispersion and the transformation into silex of the fossil woods of the Tertiary, whose specimens abound in some parts of Arkansas, Mississippi, &c., this explanation would apply as well to the silicified trunks of the Coal Measures. In any case, and though we know but little about the distribution of the vegetation at the coal epoch, we are authorized to conclude, from the former remarks, that the species of ferns predominant in the marshes of the coal were especially shrubby or herbaceous species of small size, while those of the sandy or dry solid ground were especially arborescent.

Before leaving the *Caulopterideæ* I have still a few words to say of the size of the cicatrices of their bark, compared with the diameter of their stems. These cicatrices, generally distant, placed on the stems in the spiral order two-fifths, are, when found in a good state of preservation, nearly oval or obovate and elongated at both ends, by a somewhat deep furrow. They bear in the middle the mark of a simple fascicle of vessels in the form of a horse-shoe, and the central scar is surrounded by an oval annulus. Of the two specimens formerly mentioned as having been found in the sandstone of our Coal Measures, and whose somewhat flattened stems have preserved their form as well as the cicatrices of the bark, the one, four inches in its greatest diameter, has the scars just one inch broad. In the second, three inches and a half

in diameter, the scars are not quite one inch broad. Now the largest and most remarkable specimen of a *Caulopteris* that I have ever seen and a notice of which has ever been published (*Caulopteris insignis* Lsqx.), shows a piece of bark with a single but entire cicatrice of just three inches in diameter. Admitting that the proportion of the cicatrices to the stem is, in this species, the same as in the former ones, this must have belonged to a trunk of fern of less than one foot in diameter. This agrees well with the size of the trunks of *Psaronius* of Shade river, whose diameter is mostly between four and eight inches, rarely reaching one foot.

The genus *Megaphytum* Art., should, according to Prof. Brongniart's opinion, be united with the genus *Bothrodendron* or *Ulodendron* and referred to *Lepidodendron*, as representing merely a modification of this last genus. Our American specimens do not authorize this conclusion. *Megaphytum protuberans* Lsqx., of the State Cabinet of Illinois, has the cicatrices closely placed above each other, oval, convex, with their top somewhat squarely cut at the point of junction. They bear, near the upper end, the scars of fascicles of vessels, in the form of a horse-shoe; just like the *Caulopterideæ*, but without a marked annulus. These scars were evidently left at the base of large petioles or fronds, and are not cicatrices of leaves or of adventive buds as Mr. Brongniart supposes. It is even evident, from the forms of the cicatrices, which are a little flattened at their base and more elevated at the upper part, that the fronds which were originally attached to them were ascendent and closely appressed upon each other at their base. Moreover, this species has its surface deeply and irregularly striated and furrowed as if it had been covered by rootlets, just like the surface of a *Psaronius*. The cicatrices of *Megaphytum Wilburianum* Lsqx., still more nearly resemble those of a large *Caulopteris*. They are 4 inches broad, round, or square with rounded corners, flattened, with the scars of the vessels placed in the middle, and surrounded by an annulus. From this, it appears evident, that these remarkable stems did belong to a genus of the fern family, bearing two-ranked or distichous fronds. Prof. Geinitz has already admitted the genus *Megaphytum* as intermediate between the *Lycopodiaceæ* and the ferns.

Calamitariæ.

The species of this group of fossil plants have as common characters: the stems hollow, regularly striated, articulated, with articulations more or less distant, marked by a depressed or circular ring, or by an elevated margin, bearing whorls of leaves more or less united at their base. The five principal genera of fossil plants of the Coal Measures, which have been placed in

this group, *Equisetites*, *Calamites*, *Asterophyllites*, *Sphenophyllum* and *Annularia*, have between themselves no evident and acknowledged relation. Considering the first two of these genera as belonging to the family of the *Equisetaceæ*, Mr. Brongniart has separated from it the last three, placing them with the dicotyledonous gymnospermous plants. The essential reason for this separation is, that species of *Asterophyllites* sometimes bear, in the axils of their leaves, small, flattened, oval, somewhat winged seeds, resembling those of the Yew, and, at the extremity of these branches, a kind of cone containing a pulverulent matter, which this great author considers as pollen. An inflorescence of this kind resembles that of the conifers. If we consider only the more evident characters, viz: the hollow, striated, articulated stems; the leaves more or less united at the base and placed like sheaths around the articulations, this separation appears inadmissible. It is for this reason that most of the European authors have put it aside. Nevertheless, it is evident, from good though small specimens found in our Coal Measures, that, at least, two species of *Asterophyllites* bear, in the axils of their leaves, those small oval or cordate-oval seeds, observed by Mr. Brongniart, and far different from the cones of the same genus which he considered as male flowers. It is certain also, that, from the examination of a great number of these cones, very common at some places in the shales, in connection with branches and large stems of *Asterophyllites equisetiformis* Ll., they contain nothing under their scales but a pulverulent matter, as Mr. Brongniart has seen it. Possibly the flattened seeds, in the axils of the leaves of *Asterophyllites*, could be considered as a kind of tubercles; but I really believe they are true seeds and that all the species of the genus *Cardiocarpum* are referable, if not to the genus *Asterophyllites*, at least to plants related to it. At some places where *Asterophyllites* are abundant, these seeds are seen sometimes in plenty, varying in form from round or oval to cordiform, generally bearing a narrow wing, emarginated at the top, and even broadly winged, as shown by the beautiful specimens figured and described by Dr. Newberry.[7] They vary much in size, being generally as small as a pea, but sometimes as large as a walnut. If then, as is evident, these fruits belong to *Asterophyllites*, or to plants related to this genus, it is not possible to refer them to *Equisetaceæ*, and so the opinion of Mr. Brongniart is confirmed. But now, the fruits of the genus *Calamites* are still entirely unknown. A single specimen, figured in Sternberg's *Flora*, vol. ii, pl. 14, fig. 1, under the name of *Volkmannia arborescens*, apparently coming from a stem of *Calamites*, has the form of a long ear or cone, bearing whorls of narrow, linear, obtuse, somewhat open leaves, resembling the cones of *Asterophyl-*

[7] Annals of Science, No. 13, (May 1, 1853), p. 152, No. 2.

lites and, as I believe, of the species *Ast. lanceolata* Lsqx., of the *Pennsylvania Geological Report.* The only difference is in the form of the leaves. In our American specimens they are linear, *pointed*, never obtuse as Mr. Sternberg figures and describes them. In my specimens of *Asterophyllites lanceolata* the ears are always attached to a curved, half an inch thick, articulated and striated pedicel, having just the same form as a small branch of *Calamites approximatus* Art. The form of the pedicel, curved upwards, shows that these cones were attached to the side of a large stem and not placed at the top of some branches, and thus explains the reason and the form of large cicatrices, irregularly placed above the articulations of stems of some species of *Calamites.* But species of this same genus have also smaller, round cicatrices regularly placed around their articulations. Though, according to Prof. Geinitz, these scars are left as the point of attachment of some roots, they may nevertheless be only the marks left by fruits like those of *Asterophyllites.* Thus the relation of both genera, a relation so striking, if we consider the other appreciable characters, would be complete. But, even if this affinity of forms was perfectly ascertained, the question concerning the true relative place of these plants would not be settled. For the internal structure of the *Calamites*, as far as it is known, removes them evidently from the *Dicotyledonous* and establishes their relation with the *Equisetaceæ.* It is one of those numerous dilemmas offered for a solution, to the patience and long researches of the Palæontologist.

American specimens do not add much to what was already known of the different genera of this group. I have not seen in our Coal Measures a single trace of an *Equisetites.* I did not even suppose that species of this genus could be found in the Coal Measures. The beautiful specimens figured and described by Geinitz do not leave any doubt on this question.

There is near Carbondale a forest of standing *Calamites* imbedded in a bank of compact, coarse, hard sandstone. Numerous fragments of their stems have been taken out from a tunnel cut in this sandstone. These fragments show nothing but the external surface of the stems. Even the coaly matter which sometimes covers it has disappeared. The species are *Calamites Suckowii* Brgt., *Calamites ornatus* Brgt., which Mr. Geinitz considers as the same species; *Calamites Cistii* Brgt., and *Calamites approximatus* Art. The size of the stem varies from three to six inches, rarely attaining eight inches. A number of them appear to have been crushed upon themselves when still standing, for the bark, or rather the external surface, is often pushed and folded within the stem, all around the circumference. This, of course, proves that the stems of the *Calamites* were hollow cylin-

ders, covered with a thin but strong bark. No remains at all of roots, of fruits, or of leaves, are found in this sandstone and in connection with the *Calamites.*

It is very difficult to establish the relation of the cones of *Asterophyllites* with the branches, to which they are rarely found attached, and thus to fix the true species. For this reason, I think it more convenient, though less scientific, to give different names to each of the parts of the plants, as long as they have not been found in evident connection. The roots and floating filaments, formerly known under the names of *Hydatica prostrata* Art., are now considered by Prof. Geinitz as the roots of *Asterophyllites foliosa* Lindl. They have been found attached to large stems apparently belonging to this species. The roof shales of the coal at Pomeroy, Ohio, are, in some places, covered with these radiculose filaments, and, though I have not seen them attached to the stem, the abundance of branches of *Asterophyllites foliosa*, found on the same shales, confirms the views of the celebrated German author. But Mr. Geinitz also refers the cones known as *Asterophyllites tuberculata* Ll. & H., to the same species, and these cones are not found at Pomeroy. Per contra, they abound on the shales of the red ash coal at W. W. Woods and at the Salem vein of Port Carbon, near Pottsville, where *Asterophyllites equisetiformis* is plentiful, and where I have not found *Asterophyllites foliosa* or *Hydatica.* At W. W. Woods, with numerous remains of *Calamites,* the three species of cones named *Asterophyllites tuberculata* Ll. & H., *Asterophyllites aperta* Lsqx., and *Asterophyllites lanceolata* Lsqx., are also in great quantity of fragments.

A beautiful species of *Sphenophyllum, S. bifurcatum* Lsqx., has been found in the coal inferior to the Millstone Grit of Arkansas. It may be the same species as the small specimen figured and described in the *Pennsylvania Report* as *Sphenophllum trifoliatum* Lsqx. Difficult as it is to fix the specific characters of a *Sphenophyllum,* this species, from the great number of specimens examined, may be considered as a true one. It shows that the leaves of this genus are united at the base by a narrow margin. This union exists for the leaves of *Asterophyllites* and of *Annularia;* and thus their whorls of leaves are more of the nature of sheaths, deeply cut in laciniæ of various forms, than of true leaves.

Since the time (1854) when I delivered my report on the fossil plants of Pennsylvania, I have seen nothing in our fossil plants to change my opinion concerning the fructifications of the genus *Annularia.* I supposed then, and still suppose, that these fructifications were borne on the top of the leaves, within the inflated and hollowed medial nerve, in a kind of funnel-like cavity, like the spores of some species of *Hymeno-*

phyllaceæ of our time. Prof. Geinitz, indeed, has published, in his magnificent work on the fossil plants of Saxony, as fructifications of *Annularia*, (pl. 18, figs. 8 and 9) a beautiful cylindrical long ear with an articulated and striated stem, bearing, at the articulations, whorls of short, linear, pointed leaves, and in their axils round sporanges or fruits. These fruits are undoubtedly of the same kind as those of the fragments described above, and, to my belief, belong to the genus *Asterophyllites*. Against my opinion, still is this fact: that nothing, among our recent ferns, would lead us to suppose that there ever lived species of ferns with whorled leaves. But we see, in the vegetation of the coal epoch, some peculiar features of a far more abnormal and unexplainable character. The question can be decided only by well preserved specimens. And though I have recently seen two specimens of *Annularia sphenophylloides* Ung., the one from Newport, R. I., the other from Illinois, whose appearance did perfectly agree with what I suppose to be the fruit-bearing leaves of *Annularia*, this appearance is not distinct enough to permit a positive assertion. If my supposition concerning the fructifications of *Annularia* should be confirmed, this genus would appear as a link of transition between the *Equisetaceæ* and the Ferns, as the genus *Sphenophyllum* appears to be one between the *Lycopodiaceæ* and the Ferns.

ART. XXXIX.—*On two Oceanic species of Protozoans related to the Sponges;* by JAMES D. DANA.

THE Sphærozoum figured below (fig. 1*a*) was collected by the writer in the Pacific, near latitude 30° N. and longitude 178° W., during a calm, on the 26th of May, 1841.

Figure 1*a* represents the gelatinous globule of natural size.

Fig. 1.

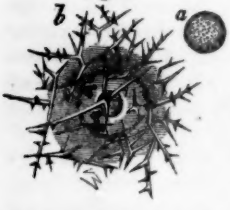


Fig. 2.

The ocean's waters were filled with this species, and another represented in figure 2*a*. The minute dots covering the globule, one of which is magnified in figure 1*b*, were closely crowded, as shown in figure 1*a*. In this respect, the species differs widely

from the figure of a species by T. H. Huxley in the *Annals and Magazine of Natural History*, xliii, 433, pl. 16; and, as it hence appears to be distinct, the writer has named it *Sphærozoum orientale*. About the dots, or ocelliform spots (zooids), the spicules (supposed to be siliceous) were very numerous and much branched as in fig. 1*b*. The general mass had an exceedingly faint bluish tinge; the centre circle of the ocelliform spots was of the same tint, while the ring around was of a very faint ochreous shade. The globules represented on the ocelliform spots in fig. 1*b* were yellow.

The other species (fig. 2*a*) had the same general color, and similar ocelliform spots as to form, color and numbers, without the spicules. Figure 2*b* represents one of the ocelliform spots; the dots in the surrounding mass correspond to minute yellow globules or cells. This species is included with the Sphærozoum under the genus *Thalassicolla* of Huxley. This name has been since restricted to Huxley's *T. nucleata*, and the name *Collosphæra* applied to forms much like fig. 2 by Müller. The mass was less firm to the touch than that of the preceding. A fuller examination of this and the related species is required to decide whether the one here figured is new or not.

Both of the species had the power of motion by a movement like expansion and contraction, and also the power of sinking and rising at will in the water. No external opening could be distinguished.

As the species are probably related to the sponges, as suggested by Huxley, they have considerable interest, and especially the Spherozoa, which, like most sponges, seem to have the power of secreting silica. The extent to which the ocean, over an area of many square leagues, was crowded with them, suggests that such floating sponges may have been, in past time, of geological importance as one of the sources of silica for the flint or hornstone and siliceous petrifactions of ancient limestones and other rocks.

These species received from the author but a partial study, as those of another class—oceanic Crustaceans—were engaging his attention at the time. The above figures and descriptions are from colored drawings made on the spot, and from the notes accompanying them.

ART. XL.—*Key West Physical Notes.*—1. *Zodiacal Light.* 2. *Atmospheric Transparency.* 3. *Gulf Stream Cloud Bank.* 4. *Ray Bands.* 5. *Northers.* 6. *Hurricanes.* 7. *Ventilation.* 8. *Yellow Fever.* 9. *A Water Moonrise;* by Major E. B. HUNT, Corps of Engineers, U. S. A.

SOME observations on physical phenomena, incidentally made by me during my period of duty at Key West, (1857–62,) may not be devoid of interest, and their discussion may have some scientific value.

1. *Zodiacal Light.*—During the winter, and especially in February, the zodiacal light habitually attains at Key West a remarkable degree of distinctness. I have repeatedly traced it nearly to the zenith, but never reliably beyond. The main point to which I would draw attention, is the great amount of light proceeding from this source. I have over and over again observed a distinct *shadow* cast by the zodiacal light. Walking from it, I have seen my shadow moving before me on the white roadway, as if cast by moonlight, though without definite boundaries. I have, by passing along close to a whitewashed wall, seen my shadow very positive in darkness, though obscure in outline. Waving my arm up and down within a few inches of the wall, a tolerably defined outline of shadow resulted. In all respects, the shadows are what should result from so diffused a light. It may be remarked that much the largest volume of light comes from the portion below 15° to 20° from the horizon. Sometimes Venus, by its brilliancy and position, rendered the observations doubtful; but I have seen these shadows unmistakably when Venus was not visible, and so late as to exclude the idea of twilight refractions as their cause. I do not know if shadows by zodiacal light have before been noted, but other persons corroborated my impressions, leaving no doubt that real, but dimly outlined shadows, of readily observable darkness, are habitually produced by the winter zodiacal light. This gives a more correct idea of its great increase of brightness on nearing the tropics, than can be conveyed by general terms. It is indeed a singularly beautiful thing, to see this grand mass of mellow light, softly fading out into the clear sky, and quite obscuring the lustre of the Milky Way by its superior brightness. Where it intersects the Milky Way, I think the two are, at the brightest, about equal in glow, but from thence to the horizon the zodiacal light so increases in radiance as to seem almost a prolongation of twilight.

2. *Atmospheric transparency.*—There is a beauty in the sky at Key West, which can hardly fail to impress even casual observers. The stars shine out with a clear lustre and fullness of numbers,

which almost exceeds the display on the brightest and coldest nights of a northern winter. It seems singular to find a climate so moistened by the Gulf Stream, still glorying in the starriest of nights. Association had made a lavish display of the starry hosts seem the peculiar prerogative of clear, cold, winter nights, and yet here they came forth, amid moisture-laden tropic airs, with a magnificence and profusion I had never seen excelled. It needed no long acquaintance with the equable climate, the nearly unvarying temperature and the steady trade winds, to see that the reason of this phenomenon is to be found in the prevalent tranquillity of the atmosphere, where it is so little influenced by contrasts of land and sea. These small keys scarcely vary the ocean conditions. I have known the thermometer at Boston pass through a longer range in one day, than in the whole year at Key West. The winds are mostly gentle and steady in direction. There are usually no conditions of great contrast and no irregular admixtures between upper and lower strata. The requisites for developing visible vapor are rarely prevalent, and I have only twice known positive fogs at Key West. However moist the air may be, if the atmosphere lacks the conditions of contrast and intermixture to make that moisture visible as vapor, the sky should seem habitually clear. Such is the obvious fact at Key West. With a climate never, even after the severest northers, below 45°, rarely down to 55°, and seldom rising to 90° in the shade, it is not to be expected that the admixture of contrasted currents should often cool to the dew point portions of this moist warm air. The equability of atmospheric conditions is thus the real reason of the rare beauty of the sky and the rich display of starry splendors, so attractive amid the soft and balmy airs of this locality, which lacks but one degree of being tropical. There is much in the quality of these nights to suggest that the astronomer would find his paradise here, but the summer mosquitoes, rain and yellow fever are rebutting facts. For winter observations, the conditions are truly admirable.

3. *Gulf Stream Cloud Bank.*—Among the striking local phenomena of Key West, is the formation, shortly before and after sunset, of a grand bank of clouds above the Gulf Stream, rising some 200 to 500 feet in prevailing height. In running along the Gulf Stream or its margins, this bank is habitually seen during the sunset hours, and a profuse atmospheric moisture is felt while sailing in the evening over the warm-water belt. Key West being about 12 miles north of the regular Gulf Stream waters, this cloud bank rises gradually along the southern horizon, stretching from E. to W. in massive and irregular fleeces, dark below and silver gilt above, under the rays of the setting sun. When the prevailing S.E. wind is brisk, this cloud bank

drifts northward and portions are often brought into the western horizon, where they are tinged with a rich red glow. The sunset scenery is in great part the result of this movement of Gulf Stream clouds, and a certain mannerism or monotony of sunset effects follows. There is much beauty of configuration and magnificence of coloring in these warm sunset clouds, but the lack of contrasting land masses detracts not a little from their variety and picturesque effects. The cloud battalions are habitually formed for these evening dress-parades, but during the morning and midday hours there is usually clear bright sunshine, occasionally mottled with a few lounging cloud-waifs which seem to drift idly and without purpose on the sea breeze. There is rarely any other marked coloration of clouds than red, orange and yellow, with simple white and dark, according to the light or shadow of the portions seen. The exclusive exhibit of warm colors in this tropical atmosphere and the glowing impression of perennial heat which the eye thus drinks in, naturally raise queries concerning the subtle affinity which couples literal warmth with warmth of color. What exquisite thermometry resides in the optic nerve, which perceptively tells us how the great source of heat has ruled the day and shall rule the morrow? It must be something more than chance which associates the heat rays of the spectrum with those primitive colors which the artist calls warm. The connection must be causal, and may be due to a positive perception of heat in the optic nerve itself. This causal impression grows into one's faith as he looks forth, evening after evening, on the fervent coloration of these Gulf Stream clouds, tropical alike in origin and promise.

The cause of the evening cloud bank along the Gulf Stream is not hard to find. During the day the sun is constantly heating up the air above the water surface, thus adding to its capacity for holding moisture in invisible suspension. With the growing heat, the point of saturation rises. The warm Gulf Stream water, under the steady radiations, vaporizes rapidly along its surface, and contributes great daily increments to the invisible atmospheric waters. When the meridian is past, and the falling sun acts with decreasing force, the atmospheric temperature declines, until, as the sunset approaches, the water laden stratum over the Gulf Stream cools to the dew point, and the invisible vapor is bodied forth in cloud masses. The superior temperature of the Gulf Stream water, by augmenting the daily evaporation, brings the air above it to the point of saturation, while the surrounding cooler waters fail so to change the adjacent air as to reach this point when the sun declines. Along the whole course of the Gulf Stream, the principle of this daily scene-shifting applies.

The famous fogs of Newport are obvious consequences of the transfer, by a wind blowing in shore, of great masses of air,

heavily charged with invisible vapor from the Gulf Stream surface. As these air masses arrive over the littoral and Narragansett waters, still cold with the accumulated cooling of the winter, their temperature rapidly sinks until the dew point is reached, and a fog results. It is in spring and early summer that this fog mechanism is perfect; but as the Bay, shore and shore waters get heated up in the advancing season, the change of temperature by shoreward transfer grows less, until in the late summer and fall, when fogs are rare.

4. *Ray-bands.*—The appearance familiarly known as "the sun drawing water" is very frequent at Key West. It is not uncommon to see the rays in the east, converging to the point opposite the sun, and as much below the horizon as the sun is above, which I will call the *anti-sun.* Sometimes the converging ray-bands in the east are nearly or quite as distinct as those in the west. The unusual frequency of these exhibitions is a result of the inshore drift of the Gulf Stream Cloud Bank. The ray-beams, through the breaks in the cloud masses, are made visible by the diffused and tenuous vapor incident to the evening cooling.

The remarkable observation on ray-bands which I wish to note, I have had occasion to make several times, when a faint haze has rendered them distinct throughout their whole course from west to east. The result is that the W. and E. systems of convergent rays visibly run into each other, producing continuous arches of light across the entire sky. The portion of each band near the perpendicular to its length was seemingly much the broadest, and the band thence tapered towards the sun and anti-sun, according to the customary perspective. Here is a notable point of singularity. So long as the W. and E. systems of ray bands are seemingly distinct, they appear to the eye as truly rectilinear and convergent. When a ray band is distinguished entirely across from W. to E., it has the appearance of a grand arch, curved in its entire extent. This is an optical delusion, caused by the mental identification of the band with the sky-dome. The observer is really placed amid a system of strictly parallel solar beams of constant cross section. The portions nearest the eye seem broadest, by reason of the greater visual angle subtended at and near the perpendicular. So long as we see only the disjoined W. and E. systems of convergent bands, we see them correctly in space according to simple perspective laws, just as when we look at the rails in a long, straight reach of railway. When however we look on a continuous luminous band across the sky, no distinctness of mental or logical conviction can make that straight band or beam in atmospheric space seem anything but a grand arch, widest near the crown, and resting on the sun and anti-sun as piers. I think it

safe to say that no clearness of geometric conception can make the eye tell a different story.

Undoubtedly we entertain the habitual, perspective fallacy of a sky-dome, truly spheroidal, with the minor axis vertical. No force of conviction prevents our seeing this dome, day by day, and thus giving it a vital reality, utterly contrary to reason. From childhood, this beautiful, phantasmal sky seems ever bending over us, and with just as much reality as the houses and forests. When we say that this sky is blue, we really mean that somewhere, not many miles away, there is a blue crystalline sphere, under which we dwell, and in which the stars are set. However perfect may be our logical conviction that atmospheric air is a blue, transparent medium, which gives us the impression of a distant blue sky-dome, we always see that dome as a reality. When therefore we see a ray band stretching from sun to anti-sun, across the face of this apparent sky-dome, we see a curvature, under the despotism of a beautiful and perennial phantasm, which has grown with our growth, until it has for us as much perspective reality as the solid ground itself. Were we to see a straight wire or timber, supported above us from the earth, and running out of sight in each direction, we should never confuse it with any sky phantasms; nor do we in the simple case of seemingly divergent ray-bands, although we find it hard to recognize true parallelism in this apparent case of radiation from a centre which we fancy to be not very remote. I think the ray-band arch is an appearance admirably fitted to teach us how great may be the delusive power of ideas which we logically repudiate but perceptively retain.

5. *Northers.*—The relaxation and enervation due to the warm and moist climate, which the Gulf Stream carries with it, is in the winter occasionally relieved by the dry, cool, exotic air of the "norther." The wind before a norther nearly always goes around by the south and west. The south wind is apt to blow one or two days with some steadiness, and I know no more debilitating and unnerving influence than the south wind at Key West. The traverse of the wind through the western quadrant is usually quite rapid. When it reaches the W. or W.N.W. point, a lull sets in, and the practiced eye looks in the N.N.W. for the rising of the "Norther Bank." A long, low, dark line shows itself above the horizon and rises with increasing rapidity, the dark mass preserving its upper margin sharply defined and horizontal. The front moves down magnificently upon us, and for a few moments, amid profound calm, we see its wild rush and hear its dull murmur. Suddenly it strikes us, and instantly all is uproar, noise, confusion, dust and darkness. Leaves and other light articles career madly, blinds are violently slammed, and it is all one can do to shut doors and windows to exclude the wild puffs of dust and leaves. Sometimes, for a few moments

there will be a dash of rain, which however speedily gives way to clear, dry, cool air. Amid all the wild inaugural ceremonies of the norther, the cool, brisk air sweeps away languor and exhaustion, and raises an effervescence of spirits which is quite equal to enjoying the mad dance, with all its dust and darkness. In a few minutes the wild humor passes, and the norther settles itself to work. Steadily it blows on from the N.N.W. or N. for a day or two, working around very slowly to the eastward. About the third day, its force is mainly spent and it shades out into a mild and delicious N.E. breeze. Still working slowly eastward, it settles at E.S.E. when the regular trades prevail for a season, until another excursion by the south preludes another circuit of the compass.

The norther of Key West is unmistakably a stratum of cold air, moving along the earth's surface from N. to S. with a flow as of a great air river. During the moments of admixture between the head of this current and the previous, warm, moist air, there is such a sudden cooling of portions of the latter, that it sends down sometimes a few dashes of rain drops, and forms the dark vaporous mass which shows in the distance as the "Norther Bank." When the current is fully established, there is no more admixture and hence no more rain, but instead a bright, clear sky and a flow of dry cool air, which braces the lungs, and brings out a crop of efflorescent crystals on the surfaces of the brick walls of Fort Taylor, making it seem suddenly gray with age. There are usually from five to ten regular northers during the winter half-year, the first coming in November and the last in March, though feeble imitations occur late and also during the winter. Last winter there was no thorough norther until March, and there is considerable irregularity about their numbers and occurrence, but, in all, the type is as above defined.

6. *Hurricanes.*—As the Key West winter has its northers, so the summer has its hurricane or hurricanes. I have witnessed but two; one quite severe and the other moderate. Mr. Redfield has so fully worked this ground, that it need only be remarked by me, that these two gales conformed to his theory of revolving storms. I here introduce two sets of barometer observations, taken at Key West during the August gale of 1861. The first was made by Mr. Charles Howe, the Collector, at the Custom House, as follows:

Date.	Barometer.		Wind.	Character of the Weather.
1861.	6 A.M.	2 P.M.		
Aug. 14,	30·50	30·46	North.	Fresh.
15,	30·30	30·28	N.E.	Very fresh at 11½ o'c. P.M. Barometer 29·94: at 1 o'c. A.M. wind shifted from N.E. to S. and blew until 5 o'c. when it commenced moderating and barometer commenced rising.
16,	30·06	30·24	South.	
17,	30·40	30·60		

"*Note.*—The thermometer during the past 3 days has ranged from 80° to 82°."

The second series was made at the Coast Survey and Smithsonian Magnetic Observatory on the Fort Taylor grounds.

Aug. 14th.	9 P. M.	29·936	Aug. 16th.	2 P. M.	29·796
15th.	7 A. M.	29·788	"	9 "	29·900
"	2 P. M.	29·700	17th.	7 A. M.	29·990
"	9 "	29·500	"	2 P. M.	30·058
16th.	7 A. M.	29·504	"	9 "	30·140

A comparison of these records shows that one of these barometers has a large constant error, but the fluctuation is alike marked in both. The sudden shift from N. to S. was followed by a rapid rising of the waters in Key West Harbor, and in the gale of 1846 this heaping up on the south side of the Key amounted to about 7 feet.

7. *Ventilation.*—The close neighborhood of the Gulf Stream renders the air of Key West peculiarly warm and moist. This makes free ventilation and shade the chief essentials for all personal comfort. A peculiar difficulty exists in the preservation of all kinds of perishable articles of food, the combination of warmth and moisture being the very condition for rapid decay. I think there can be but little doubt that, for many articles, the correct plan for preservation is, to seal them up in close, shaded chambers, in which the air is kept as dry as possible. An experiment which I made on the preservation of flour, in a room opening at top into the Fort Taylor bakery, and the air of which was thus kept artificially dry, indicated that flour could there be kept sweet at least twice as long as when stored in a very dry, wooden storehouse, which would usually be chosen as the very best storage. I have no doubt that the legitimate method of keeping powder magazines dry there, is by totally excluding all ventilation. A magazine free from leakage, once filled with dry powder, with the air once dry and then sealed hermetically, would remain utterly unchanged and the powder could not get any moisture to absorb, hence it must perforce keep dry. By the use of chlorid of calcium or other moisture absorbents, or by the induction of occasional changes of heated air, all moisture could be kept from approach to the powder. If we admit free ventilation, we furnish a constant supply of moisture for absorption. The effect of opening ventilators in the Fort Taylor magazines is sometimes actually to *wet* the floor and other surfaces on which the moist air blast is thrown. The interior of the magazine is enough cooler than the outer midday air to cause an active deposition of moisture; so that the nearly saturated noon and afternoon air is the worst of all in its effects. There seems to me but little doubt, that a careful study of physical principles, in their application to the preservation of supplies,

in store at Key West and other like positions, would reverse much of the existing practice, and would enable us to preserve for a long time the stores which are now so speedily ruined by moisture. The adoption of closed inner chambers, artificially dried, with an exterior ventilation, under the roof and within the outer walls, to keep down the temperature, would add enormously to the durability of perishable supplies, and to the dry storage of gunpowder or other moisture-absorbing stores. These views have unfortunately had but little chance of practical test, except in the instance of flour storage already cited. Their great importance in their application to such public stations as Key West, the salvation of which may turn on the preservation of flour and other perishable stores, would certainly justify a most careful experimental research under the strict guidance of scientific indications.

8. *Yellow Fever.*—I will venture here to introduce a singular and significant observation, concerning the characteristic disease of tropical shores. On two separate occasions, when there were cases of yellow fever in the U. S. Marine Hospital, which building I passed daily and saw almost habitually, I have seen a flock of buzzards, circling over and near the roof of the hospital by the hour together, and continuing this day after day. I have never seen them do this except when there were yellow fever cases in progress under the roof. So marked is this fact, as to have produced a common belief in town, that they only hover over the hospital when there is yellow fever there. I am quite persuaded that such is the fact, and can only interpret what I have myself seen as indicating that an odor is then thrown out on the air which the keen scent of the scavenger bird detects from afar. The material particles, whose diffusion is thus testified to, seem likely to afford the means of transporting the disease on the air, in a manner quite agreeing with the facts of its propagation. The hint, thus afforded by the keen-scented buzzards, may have value in assisting to comprehend the mode of conveying and diffusing this fatal malady, and the particles scented may indeed be the actual *fomites* so much talked of and so little understood, in discussing the controverted questions of contagion and communication.

9. *A Water Moonrise.*—When becalmed in a beautiful evening between the Reef and the Key, the water being very tranquil, I saw the moon rise over the sea with some interesting appearances. The long reflection of the emergent disc on the water was well defined, and seemed to be a part of the moon itself. As the under semicircle of the disc began to rise above the water, there was an appearance of drawing in at the sides of the combined luminous figure. As this seeming contraction progressed, the outline showed a curved figure, like that made by water in

raising a cohering disc from its surface. There was no cusp point between the disc and the disc-reflection, but a seemingly distinct curve, concave outwards. As the disc rose above the water, this curve opened, and a broad connecting column seemed to bind the disc and its reflection, just like a coherent water column between the lifted disc and the level water surface. Instantly this seeming column parted as if broken, when the moon was seen to be distinctly above the water by about a fourth of its diameter, as nearly as I could estimate. The sudden shock of rupture appeared perfectly distinct, and the semblance of a material connection between the disc and reflection was perfect, both before and at the instant of visible separation. This observation has interest in its relation to the contact phenomena of eclipses.

ART. XLI.—*Observations upon some of the Brachiopoda, with reference to the genera Cryptonella, Centronella, Meristella, and allied forms;* by JAMES HALL. Abstract of a paper read before the Albany Institute, February 3d, 1863.[1] (Communicated by the author.)

IN the study of the Palæozoic Brachiopoda, we are often forced to rely upon the general external form, and texture of the shell, for determination of the generic relations, until more extensive collections may furnish us with weathered specimens, or with crystalline or silicified ones, which, admitting of being cut, and macerated in acid, will enable us to ascertain the true interior characters.

In many instances, so nearly do very distinct genera approach each other in their external form, that reliance on this alone is very uncertain, and will surely lead to much confusion, if insisted upon as the means of generic determination.

For a long time, and until we began to learn something of interior structure, a large number of species, now known to belong to distinct genera, were embraced in the designations *Terebratula* and *Atrypa*. At a later period, when the genus *Rhynchonella* had been established in its application to many Palæozoic species, we find numerous species, which from external form had been referred to that genus, possessing characters incompatible with it.

One of the most common of these is *Terebratula cuneata* = *Rhynchonella cuneata* = *Retzia cuneata*, and which will probably

[1] From the Transactions of the Albany Institute, with some verbal corrections and the introduction of subsequent observations by the author.

be found to differ from true *Retzia*, taking its place near *Rhynchospira*.

So long as we remain unacquainted with the interior of the shell, we are compelled to refer the species to some genus having similar external forms, though the fibrous or punctate texture may in many instances prove a valuable aid in these references.

Among the forms most difficult to determine, are the numerous smooth or finely striated terebratuloid shells, having either ovoid, elongate, sub-circular or transverse forms. Among the genera of one family which in recent times have been established and proposed to receive these, are *Athyris* (= *Spirigera*), *Merista* (= *Camarium*), *Meristella* and *Charionella;* while the subdivisions of the terebratuloid forms in another direction have given *Terebratula* proper, *Terebratulina*, *Waldheimia*, *Terebratella*, *Centronella*, *Cryptonella*, *Rensselæria*, etc.

The first four are of the athyroid type, and have internal spires, as in *Spirifer*. *The shell in all these is fibrous, and we have therefore in the external shell the means of separation from those of the other type.*

In all the latter group we find modifications of the internal appendage, called in *Terebratula* the loop; but in none of them do spires exist. Moreover, *in all these the external shell is punctate;* and we do not yet know a punctate shell, of the external character here indicated, which contains internal spires.[3]

The external characters, therefore, of the terebratuloid forms may be made useful in indicating the family relations of the species, and may prevent us from referring to the family of *Spiriferidæ* those which belong to the family of *Terebratulidæ*.

In the *Thirteenth Report on the State Cabinet*, published in 1860, I proposed the name of *Meristella* for certain forms which I regarded as separable from *Athyris* and *Merista;* and for the semi-plicated forms otherwise of similar character, I suggested the name *Leiorhynchus*. At the same time I described under *Terebratula* the following species: *T. Lincklæni*, *T. rectirostra*, *T. Lens* and *T. planirostra;* under each one, distinctly stating the shell structure to be punctate, which character at that time afforded me the principal means of distinguishing these from athyroid species of similar form, as *Meristella Haskinsi*, *M. Barrisi* and *M. Doris*, which, with *Atrypa scitula* (*4th Dist. Report*) = *Meristella scitula*, have at a later peroid been placed by Mr. Billings among the typical forms of his Genus *Charionella*.

Having ascertained some farther characters of these punctate Terabratuloid shells, I proposed in the *Fourteenth Report on*

[3] The plicated forms of *Retzia* and *Rhynchospira* are of course not included in the designation above made. *The Nucleospiræ* also approach the terebratuloid forms, but these shells have an area on the ventral valve and a different hinge structure.

the State Cabinet,[3] page 102, the name *Cryptonella*, giving as one of the characters "shell structure finely punctate." I remarked in a concluding paragraph:

"The species of this genus are more elongate than *Merista* and *Meristella*, and those now known are less distinctly marked by mesial fold and sinus; while the beak is more attenuate, often a little flattened, and rarely so closely incurved as in the genera cited. The punctate structure of the shell is a distinguishing feature."

In the *Fifteenth Report on the State Cabinet*, I gave (at page 161 [133], pl. 3) some illustrations of the muscular imprints, dental lamellæ, etc., with figures of a single additional species from the Lower Helderberg group.[4]

[3] Made to the Legislature April 10th, 1861, and published in July, 1861.

[4] In the *Canadian Naturalist and Geologist* for October, 1862, we find the following exposition of the relations of the genus *Cryptonella*:

"The genus *Cryptonella*, illustrated on pl. 3, p. 133, is precisely identical with *Charionella*, described by me in the *Canadian Journal* of March, 1861, p. 148, and illustrated in the May number, pp. 273, 274. It includes the species described by Prof. Hall in the Thirteenth Report under the names of *Meristella Haskinsi*, *M. Barrisi*, *M. Doris*, *Terebratula Lincklæni*, *T. rectirostra*, *T. Lens* and *T. planostria*, [*T. planirostra*]. Besides these, the *Atrypa scitula* of the New York Reports, *C. Circe*, and apparently a number of European species belong to it. *Cryptonella* was first published in July or August, 1861, three or four months after the learned author became acquainted with its characters through the study of my papers."

The following is the description of the genus *Charionella*, copied from the *Canadian Journal* (March, 1861), No. XXXII, p. 148:

Genus CHARIONELLA. "Since the foregoing article on Devonian fossils was written, I have ascertained the generic characters of the so-called *Atrypa* or *Athyris scitula*. It has internal spires with their apices directed outwards, as in *Athyris* and *Spirigera*, but the dorsal hinge-plate has its anterior margin and a large portion along the middle anchylosed to the bottom of the valve. In another congeneric species, the middle portion of the same plate is obsolete, there remaining only two small, thin, nearly vertical septa (socket plates), one on each side of the cavity of the umbo. The perforation in the beak of the ventral valve is bounded on the lower side by a deltidium of either one or two pieces, or by a portion of the shell. The mesial septum in the dorsal valve is either rudimentary or entirely absent.

"The several species of this group, at present known to me, resemble *Athyris*, but are not so convex, and are besides more elongate ovate, or approaching to *Terebratula* in general form. I shall give further details and some figures in the next number of the *Journal*.

"The genus is only proposed as a sub-genus, to be retained in case *Athyris* is divided."

In the *Canadian Journal*, No. XXXIII, p. 273, we have "*Charionella Circe*, n. sp." (referring to the illustrations). "The first figure exhibits a specimen with the dorsal valve partly removed, showing *the internal spires*. The other two figures are a side and ventral view of another specimen."

"By treating partially silicified specimens of this genus with acids, I have ascertained that the structure of the hinge plate differs from that of Spirigera in being either obsolete along the middle or anchylosed to the bottom of the valve. In *Athyris* (=*Meristella* Hall) there is a well developed hinge plate, supported beneath by a strong mesial septum, which extends sometimes nearly to the front of the valve. In *Charionella* there is either no mesial septum, or one that is merely rudimentary. In one specimen there is a remarkable partition, which runs obliquely from near the beak to the margin near the front. It completely divides the internal cavity into two parts. This I believe to be not a mesial septum, but a

In September, 1862, Prof. A. Winchell, in his "*Descriptions of fossils from the Marshall and Huron Groups, of Michigan*," published a description of *Centronella Julia*, in which he describes the loop, which is proved not to be in accordance with that of *Centronella* as described and illustrated in the *Canadian Naturalist and Geologist*, vol. iv, April, 1859.

Through the kindness of Prof. Winchell, I have been put in possession of some specimens of this species, with parts of others illustrating the internal structure, together with drawings representing the loop.

An examination of the external characters shows that the shell has the form and texture of *Cryptonella*. "Both valves with regular lens-like convexity, shell obsoletely striate concentrically, and having a minutely punctate structure." The form and other characters of the cast are like those of species referred by me to *Cryptonella*. In the ventral valve are two delicate, slightly curving dental lamellæ, which are shown in casts by a narrow slit on each side of the beak. "The casts exhibit on the ventral side a delicate impressed line extending from the beak to the middle, and on the right and left of this a fainter one; on the dorsal side, a median impression, with two fainter ones on the right and two on the left." These characters appertain to the casts of *Cryptonella* (see fig. 9), as shown in the ventral side of large individuals; having three defined, slightly impressed spaces, limited by narrow lines which extend to the middle of the shell, below which there are sometimes vascular impressions visible.

On the dorsal side, we have the median impressed line with two fainter ones on each side, which, in some conditions of preservation, are obscured by the muscular impression; and below these are frequently seen diverging vascular impressions.

The internal loop of *Cryptonella Julia*, illustrated from drawings of Prof. Winchell, is shown in figures 1 and 2, which are four times enlarged, and are thus described: "A delicate ribbon-like loop originates from the stout blunt crura on each side of the socket-valve, having its flat sides at first vertical; the two branches of the loop proceed at first in lines parallel or a little convergent, and then gradually diverge, widening as they proceed, and assuming an inclined position, until, approaching the front of the valve by a regular curvature, the lower edge has become anterior, giving the band an angle of 30° with the plane

temporary wall formed by disease of the animal, *because both spires are crowded into the smaller of the two cavities, the larger being empty.*"

The genus *Charionella*, therefore, clearly belongs to the *Spiriferidæ*, and the typical species cited are, in part those originally placed by me under the genus *Meristella*, in 1860 (*Thirteenth Report on the State Cabinet*, p. 84), and in part under *Terebratula*, from the characters of which I proposed the genus *Cryptonella* in 1861. The former belong to the *Spiriferidæ*, and the latter to the *Terebratulidæ*.

of the shell: approaching the median line, the band rapidly widens, and the front margin is drawn forward in a long acumination, while the inner margin is regularly concave, except that near the median line it turns abruptly forward so as to meet that line at an acute angle. The loop thus forms an urceolate figure on its inner margin, and on the outer a somewhat oval one, truncated behind and attenuately acuminate before. In the median line where the two branches meet, both are suddenly deflected downwards, forming a double vertical plate, not quite reaching the ventral valve; the upper edge of which, when viewed from the side, is flatly roof-shaped, while the lower edge describes two convexities, the greater anterior, leaving a notch between them. The surfaces of the loop and median plate are covered with minute obliquely conical pustules, in some places seeming to become spinulous."

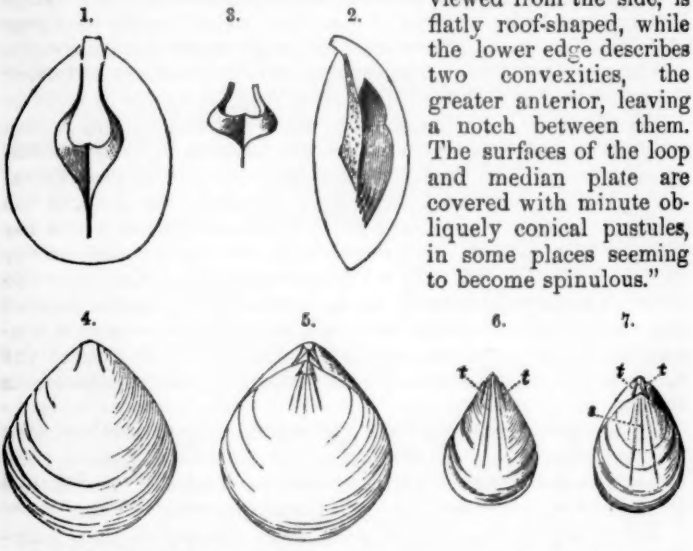
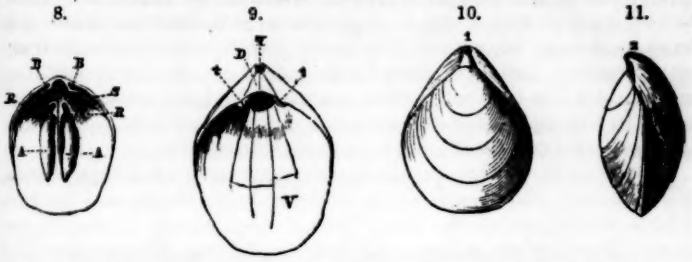


Fig. 1. Dorsal view of *Cryptonella Julia*, showing the loop and horizontal plate. —Fig. 2. Profile view showing one band of the loop with the vertical plate. From drawings, four times enlarged, by Prof. Winchell.—Fig. 3. Front view of the loop. —Figs. 4 and 5. Ventral and dorsal views of the cast of a more oblate form of *C. Julia* enlarged to correspond with figs. 1 and 2.—Figs. 6 and 7. Ventral and dorsal views of *Cryptonella Meta*, from the Schoharie grit.

Fig. 4 is given simply to show the dental lamellæ of the ventral valve; the delicate impressed line in the centre and a fainter one on each side, described by Prof. Winchell, are not shown in the figure. These marks, however, are shown in figs. 6 and 9, and characterize the ventral valves or casts of this valve in all the known species of the genus.

In the *Fifteenth Report on the State Cabinet*, I gave the accompanying fig. 8 of the dorsal valve, and fig. 9 of the interior of a ventral valve. Figures 10 and 11 are dorsal and profile

views of *Cryptonella eximia*, from the Lower Helderberg group, the earliest species of the genus known to me.



Figs. 8 and 9. Dorsal and ventral views of *Cryptonella* (generic illustrations).—Figs. 10 and 11. Dorsal and profile views of *C. eximia*.

The genus *Cryptonella* may be characterized as follows:

GENUS CRYPTONELLA *Hall*, 1861.—Shells terebratuliform, equilateral, inequivalve, elongate or transverse, ovoid or sublenticular in form, without median fold or sinus, or with these features very slightly developed towards the base of the shell. Ventral valve with the beak extended or incurved, and terminated by a circular foramen which is limited on the lower side by two small triangular deltidial pieces (these are sometimes not visible externally, and the lower side of the foramen is concealed by the umbo of the opposite valve). Shell-structure finely punctate; surface marked by fine concentric striæ, which are sometimes obsolete. Valves articulating by teeth and sockets, the dental lamellæ of the ventral valve extending in thin vertical plates into the cavity of the valve. The muscular impressions of the dorsal valve are strongly marked above, and extend, in two narrow, gradually widening impressions, more than halfway to the base. The ventral valve shows elongated muscular and vascular impressions below the rostral cavity.

Cryptonella.

In the dorsal valve, the hinge-plates, or bases of the crura, support a slender loop, the two limbs of which are flattened, with the faces vertical; and in its extension forward, the upper margins are inclined towards each other, and, gradually widening, become joined, and thence extending forward, form a single lanceolate plate, which may be more or less attenuate in front. These laminæ of the loop, after becoming thus conjoined and spreading laterally, are abruptly deflected in a vertical plate along the median line, extending into the cavity of the ventral valve, as shown in figure 2, which, while looking upon the dorsal side of the loop, may sometimes

be seen projecting backwards between the bands of the loop, as well as extending in front, as shown in fig. 12.

In casts of the ventral valve, we find the marks of two thin dental lamellæ extending to a greater or less distance below the beak. Along the median line in the ventral cast, there is usually a narrow flattened space limited by a slender line; and on each side a less distinct narrow space, limited in the same manner. In the cast of the dorsal valve, there is a median impressed line, and two of less strength on each side of this.

The species of this genus, known to me, are the *Cryptonella* (*Centronella*) *Julia*, and those described as *Terebratula* in the *Thirteenth Report on the State Cabinet*, and which in the *Fourteenth Report* were referred to *Cryptonella*, viz. *Cryptonella* (*T.*) *rectirostra*, *C.* (*T.*) *Lens*, *C.* (*T.*) *planirostra;* and *C. eximia*, of the *Fifteenth Report* as well as a new species from the Schoharie grit.

The *Terebratula Lincklæni*, which has the external characters of *Cryptonella*, and which I have referred to that genus, presents some slight differences in the muscular impressions, which, taken together with its rotund form, are suggestive of true *Terebratula*, to which genus it may possibly belong.

The species of the genus *Centronella* heretofore described have the ventral valve highly convex or subangular in the middle, with the dorsal valve flattened or concave in the middle, or with a median depression, and convex at the sides.

The character of the genus, as given in the descriptions and illustrations of Mr. Billings, are as follows.

Genus Centronella, *Billings*,[5] 1859.—"Generic characters: Shells, having the general form of *Terebratula*. Dorsal valve with a loop consisting of two delicate ribbon-like lamellæ, which extend about one-half the length. These lamellæ at first curve gently outwards, and then approach each other gradually, until at their lower extremities they meet at an acute angle; then, becoming united, they are reflected backwards towards the beak in what appears to be a thin flat vertical plate. Near their origin, each bears upon the ventral side a single triangular crural process. Name, from the Greek κεντρον, a spur. This genus is intermediate between *Terebratula* and *Waldheimia*. In the former, the loop is short, not exceeding greatly one-third the length of the shell, and not reflected. In the latter, it extends

13. 14.

Fig. 13 (4). Interior of the dorsal valve, showing the loop.—Fig. 14 (5). Longitudinal section, showing the position of the loop in the interior.

[5] Description and figures copied from the *Canadian Naturalist and Geologist* for April, 1859: the figures enlarged three diameters.

nearly to the front, and is reflected, but the laminæ are not united until after they are folded back."

In *Centronella*, as thus illustrated, we have a simple loop, or the two limbs becoming united at an acute angle at the point of greatest anterior extension, whence they recurve in a thin vertical plate which is not attached at either margin; approaching, in some respects, to *Waldheimia*.

This internal feature is accompanied, in the cast of *C. Glans-fagea*, the typical form of the genus, by other differences which distinguish it from the casts of typical species of *Cryptonella*.

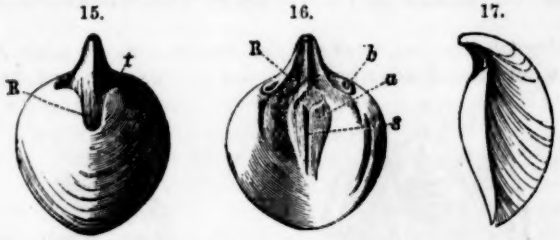


Fig. 15. Ventral view of cast of *Centronella Glans-fagea*.—Fig. 16. Dorsal view of cast of the same.—Fig. 17. Profile view of the same.

In the cast of a ventral valve of *C. Glans-fagea*, fig. 15, we have the filling of a deep rostral cavity; the dental lamellæ have been thick and strong, not extending as thin plates into the cavity of the shell as shown in several species of *Cryptonella*, but having a thick blunt lower termination which leaves no space, or scarcely an appreciable one, to be filled between it and the shell. The spur, or filling of the rostral cavity, is striated; at its base in the centre, on the body of the cast, is a depression; and on each side are fainter striated impressions, indicating the points of muscular attachment.

The interior of the ventral valve of *Centronella impressa*[6] shows similar strong rounded and blunt dental lamellæ, with a deep rostral cavity and muscular markings, which would give a cast similar to that of *C. Glans-fagea*.

The cast of the dorsal valve of *C. Glans-fagea* presents a slightly concave surface, and on each side of the apex two large and deep cavities made by the bases of the crural processes; and between them is a narrow filling of stone. The centre is marked by a double muscular impression, the two parts separated by a narrow groove: above this, and at the base of the crura, are some points marked as if for muscular attachment (see *b*, fig. 16).

[6] A very distinct species from *C. Hecate* (Billings) of the Oriskany Sandstone, which differs mainly in size from *Centronella* (*Rhynchonella?*) *alveata* (Hall), *Tenth Report on the State Cabinet*, 1857.

The interior of *C. impressa* presents a very strong double process below the beak of the dorsal valve, corresponding to those in *C. Glans-fagea.*

The external form of all the species heretofore referred to *Centronella* is a distinguishing feature, and, when proved to be accompanied by an internal apparatus so different from that of *Cryptonella*, will serve to separate them from all the allied forms.

As before remarked, it has been mainly upon modifications of this internal loop, or the apohysary system, that the separation of most of the genera in the family of *Terebratulidæ* has been made.

In *Cryptonella*, we observe considerable analogy with *Rensselæria*, where the slender bands of the loop expand and unite in

Fig. 18. Dorsal valve of *Rensselæria Suessana*, showing the internal processes.—**Fig. 19.** Longitudinal section of the same, showing the relations of the parts.—**Fig. 20.** Interior of dorsal valve of *R. ovoides*.—**Fig. 21.** Longitudinal section of the same.

a broad plate, which is obtusely or acutely attenuate in front, and on the ventral side marked by a ridge along the line of junction; from which, at the posterior margin, proceeds a slen-

der process in the ventral cavity. We may readily conceive of this central longitudinal ridge or carina, along the cicatrix of the two parts, being produced into a thin vertical plate, projecting backwards in the line of the process from the base of the conjoined lamellæ in *Rensselæria*, when it would much resemble the median plate of *Cryptonella* (see figures 18, 19, 20 and 21.)

From the data here given, it will be seen that the genus *Cryptonella* is nearly related to *Centronella;* differing in the external form of the typical species, and in some features of the cast.

Since the preceding observations were printed, I have received from Dr. Rominger a figure illustrating the interior of *Centronella Glans-fagea*, as observed by him (fig. 22). Admitting the identity of the species, this figure of the loop is quite different from that given by Mr. Billings for *Centronella Glans-fagea;* and shows essentially the same character as that of *Cryptonella.* Should this internal structure prove to be the true structure of *Centronella*, the minor differences pointed out in the form of the shell and of the cast, between *Centronella Glans-fagea* and authentic *Cryptonella*, are scarcely sufficient to establish generic distinctions.

22.

Centronella Glans-fagea.
Interior showing the loop, from a figure by Dr. C. Rominger.

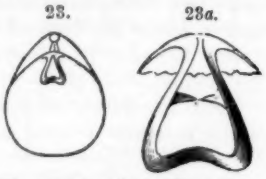
At a later date however,[6] Mr. Billings has published *Centronella Hecate*, giving, in fig. 99*a* "a specimen with the dorsal valve removed, showing the loop which is covered with minute crystals of silex." In this species, having all the external characters of a congener of *C. Glans-fagea*, no indication is given that any difference had been observed in the character of the loop, from that published in 1859.

Under these circumstances, I hesitate to unite, under a single generic term, these varieties of form with an internal structure so different from that observed in authentic *Cryptonella*, until a reëxamination of the original specimens of Mr. Billings shall confirm his first observations, or show them to correspond with the last named genus.

It is not probable, however, that materials for other genera, or for reference to existing genera, are yet exhausted, among the *Terebratulidæ* of the Upper Silurian and Devonian rocks. While engaged in these investigations, Dr. C. Rominger has kindly sent me a fossil from the Hamilton shales, of Thunder Bay, Michigan, in which the terebratuloid loop is distinctly visible. The form of the shell is ovate, not very unlike *Cryptonella*, but more rotund, the lateral edges more incurved, and the space

[6] Canadian Journal, May, 1861, p. 272.

below the beak of the ventral valve not so great, nor the deltidial plates so conspicuous as in species of that genus. On a critical examination of the interior, after cutting away the crystalline filling of the shell nearly to the loop, I am unable to find any difference between it and true *Terebratula;* and we have, so far as I know, for the first time the positive determination of this genus in our Devonian rocks. The position and proportions of the loop are shown in fig. 23, which is an outline of the shell from the dorsal side, twice enlarged. Fig. 23*a* is an enlargement of the loop, showing the crural processes.



Figures 23 and 23*a*, Illustrations of *Terebratula Romingeri* Winchell.

At the same time, Dr. Rominger has also sent me specimens of *Terebratula melonica* of Barrande, one of which he has prepared so as to show in a very satisfactory manner the loop in its entire extent. The specimens correspond with those I have received from M. de Verneuil under the same name, and therefore we must regard them as authentic. The external form of *T. melonica* is not unlike some of the less gibbous of *Cryptonella*,

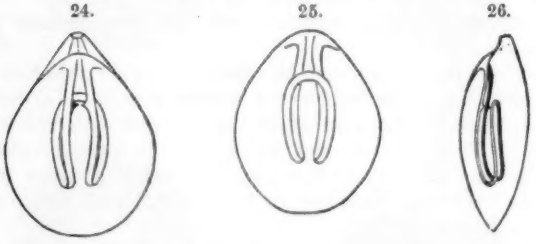


Fig. 24. Dorsal side of *T. melonica*, showing the crural processes directed downwards.—Fig. 25. Ventral side of specimen, looking into the dorsal valves.—Fig. 26. Profile view of same, the figures twice enlarged.

and is much less gibbous than the usual forms of *Waldheimia.* The lamellæ are nearly parallel and near together, and the loop is extended four-fifths the entire length of the shell, when it is recurved, and, turning back, extends two-thirds of the distance to the beak of the dorsal valve; and the crural processes are farther from the base of the loop than is represented in the typical figures of *Waldheimia*, and are opposite the extremity of the recurved loop.

The above figures illustrate all that has been observed in this species.

(*To be continued.*)

Art. XLII.—*Scientific Correspondence.*

I. *Letter on Companion to Sirius, Stellar Spectra and the Spectroscope*, from Lewis M. Rutherfurd, dated 175 Second Avenue, New York, March 31, 1863.

Gentlemen:—

1. *Companion to Sirius.*—The position and distance of the companion of Sirius has been measured at my observatory, this season, with the following results. Seventy-nine measures of position, in all, have been made, on six different nights, of which the mean epoch is March 14, 1863; the mean position obtained is 81° 21′ 45″. Thirty-eight measures of distance have been made, the mean result of which is 9″·54. Last year, the position resulting from a mean of forty-eight measures, on six nights, mean epoch March 28, was 84° 58′ 46″, while twenty-eight measures of distance gave 10″·09. From a comparison of these results, it appears that, while the change in distance, 0″·55, is so small that its existence cannot be asserted with confidence, a marked change of position has taken place, amounting to 3° 37′, a quantity so decided that the motion may be taken as fully established: at this rate of motion, assuming it to be circular and in a plane perpendicular to the line of sight, the little star would complete a revolution in about 100 years, or, I believe, twice as long as the period ascribed to the excentric motions in declination of Sirius. I hoped to have been able to compare the direction and quantity of motion detected with the orbit attributed to the opaque body supposed to disturb the great star, but I have been unable to lay my hand upon the papers of Bessel and Peters upon this subject, in time for this letter. I still wonder that Clark's great little star has so long escaped detection; it is a much less difficult object than Mimas, and never fails to show itself in my telescope on any moderately good night. I saw it distinctly in February, with a telescope of nine inches aperture and nine feet focus, made by Mr. Fitz, formerly owned by me, and now belonging to the Hon. Mr. Letsom, British Consul at Montevideo.

2. *Stellar Spectra.*—Since writing to you in December (p. 71, this volume), I have mounted my astronomical spectroscope in a more firm and convenient manner; I have added a prism, by means of which the spectrum from a spirit lamp is constantly present in the field of view, during the observation of a star: I find this a most useful check, and by means of this comparison I have established the existence in the spectrum of Arcturus of the lines D, E, *b*, and G, and, almost with certainty, found that each line in the spectrum of the star has its counterpart in the solar spectrum.

3. *The Spectroscope.*—I have employed the bad weather, this winter, in the construction of a large spectroscope, telescopes 20 inches focus and 1·6 aperture; the prisms, of which I have so far used but six, are hollow cases of brass cast in one piece, with their faces carefully ground, upon which are cemented plates of glass, originally made for shades for artificial horizons, and consequently nearly plain and parallel; I say nearly, for I have yet to find one square inch of plain and parallel glass; these prisms under certain conditions perform beautifully; the obstacles

to fine performance are two-fold. 1st. I find no specimen of bisulphid of carbon homogeneous in density: upon shaking or disturbing the position of the prism a violent agitation of the image occurs, and in examining it without the eye-piece, after the mode adopted in detecting veins in an object-glass, the whole interior of the prism is seen full of waves and striæ, presenting the appearance of alcohol and water not yet thoroughly mixed; this trouble is cured by time, from a quarter to a half hour being a sufficient rest. 2d. The brass frame is so much more affected by temperature than the glass plates that any great variation is destructive of good definition. This is, I fancy, the cause of the distortion of surface observed by Prof. Rood, rather than the warping effect of the glue. I propose to cure the evil by constructing the frame of solid glass; but, although it is a simple triangular block pierced with an oblong hole, I have not yet found a glass-maker adventurous enough to undertake its construction. I shall persevere, however, for I prefer this remedy to the ingenious plan, adopted by Prof. Rood, of applying additional plates of glass, the surfaces being separated by a thin film of fluid which will not communicate to the outer plate the distortions of the inner: the objections to this plan are that it is complex, the fluid is liable to exude or accumulate in greater thickness at the lower edge of the plates, and, above all, the difficulty of obtaining thin glass with true surfaces.

[The trouble mentioned by Mr. Rutherfurd is probably due to the high coefficient of expansion of CS_2, which renders it so sensitive to changes of temperature that simply handling the prisms will disturb the uniformity of density in the manner noticed by Mr. R. In a large spectroscope constructed by Mr. Alvan Clark of Cambridge or Prof. Cooke, from the plans of the last-named gentleman, eight CS_2 prisms on iron frames are used, with Prof. Rood's plan of glass cover plates. These plates are polished with the greatest care, and give results quite satisfactory. Prof. Cooke has also succeeded in obtaining, from the New England Glass Works, glass triangular frames in one piece with an oval opening, from which Mr. Clark has prepared CS_2 prisms, holding nearly a pint of liquid, and exposing faces of about five inches length by three high; two of these project a spectrum from Deleuil's electric lantern with great intensity, fourteen feet long, in which the inversion of the D line by vapor of sodium (mentioned on p. 414 of this volume) is very effectively shown.—B. S., JR.]

4. *Analysis of the Sodium line D.*—As I said above, my brass prisms under favorable conditions perform admirably; with six of them I am confident that I have seen the line D composed of nine (see figure); this diagram is rude, not founded upon measures, but merely a copy of a sketch made when I first saw the lines; the three on the right of Kirchhoff's central line are not difficult, being readily seen with three prisms of 60°, of bisulphid and one of 45°, of glass (it not being possible to use four of 60° on account of the interference of the telescopes). Of the three in the left compartment, the central one is the most difficult, and all require the best adjustment and light.[1]

[1] Since writing the above, I have entirely confirmed the correctness of this diagram, having at one time used eleven prisms. — R. April 17th.

The line B is resolved into fourteen fine and close lines, with a beautiful and symmetrical band of finely doubled lines stretching towards A; I think it the most beautiful part of the spectrum. A broad band of fine and close lines adjoins A on the least refrangible side, somewhat resembling the neighborhood of B, and I am confident that A itself is composed of fine lines. In the potash spectrum, I have found some peculiarities which I have not seen mentioned: beginning at the least refrangible end we have the first line A boldly double; a little short of the place of the red lithium line is another pair not quite so wide as the first, which flash but for a moment; close upon the green side of the soda line is a group of four lines, three quite strong and one faint; further on in the green is another group of three lines, and finally the violet line β is double, about as widely separated as A. I have not yet measured the places of these lines, but will send you the results when obtained. The orange strontium column is beautifully resolved into close and fine lines. I am very truly yours,

LEWIS M. RUTHERFURD.

II. *On the origin of the nitrites, &c.*, in a letter to the Editors from Prof. GEO. C. SCHAEFFER, dated Washington, D. C., March 18th, 1863.

Gentlemen: In the last number of your Journal, p. 271, there is a letter from T. Sterry Hunt, F.R.S., on the theory of nitrification depending upon the formation of nitrite of ammonia from water and atmospheric air. This letter requests, as "an important part of the history of this subject, and especially as an explanation of the theory of the reaction," the reproduction, from the "*L. E. and D. Philos. Magazine*, for January, 1863," of the "translation of a note *On the nature of Nitrogen and the theory of Nitrification*," read by Mr. H. "before the French Academy of Sciences, on the 15th of last September."

Mr. Hunt also says, "My object is to claim for myself the new theory of nitrification, which Schönbein seeks to found upon his recent experiments, and which I published nearly two years since."

As an humble worker in the cause of science, I would also ask permission to contribute my mite to the history of this subject.

In the *Annual Report of the Smithsonian Institution* for the year 1861, there is (p. 305) a *Report on Nitrification presented to the Smithsonian Institution in* 1858 [1856][1] by Dr. B. F. Craig, in which the following passage occurs:

"Viewing the subject by the aid of such lights as science affords, the hypothesis which appears to be best in accordance with the facts known, concerning the combination of oxygen and nitrogen, is that propounded by Dr. G. C. Schaeffer, which is based upon that general chemical action by which various bodies assume the elements of water in such a way as to produce salts of ammonia. This action takes place very commonly

[1] Since this letter was written, the following note has been furnished by Dr. Craig:

"March 29th, 1863.

"The date appended to my paper on Nitrification, which was published in the *Smithsonian Report* for 1861, is a misprint. The real date on the manuscript is 1856. I did not correct the proof, a circumstance which will account for the occurrence of this and a few other typographical errors. B. F. CRAIG."

with those substances which are produced from ammoniacal salts by the *separation* of the elements of water, and may be effected under the influence either of acid or of alkalies, and sometimes by the action of water alone at a high temperature.

"Nitrous oxyd (NO) will generate the nitrate of ammonia by the assumption of the elements of water; for, by the action on it of water and potash at an elevated temperature, ammonia is evolved and nitrate of potash formed, showing that there has been a production of nitrate of ammonia, and a subsequent decomposition of it by the potash. [There are numerous substances which are formed from salts of ammonia by the separation of the elements of water, and which will regenerate the salts by reassuming them. They are known to chemists as amids, anhydrids or nitryls]. Supposing nitrogen to act in the same way, viz: to assimilate four equivalents of water, it will form *nitrite* of ammonia, which, by a well known tendency of the nitrites, will pass into the condition of a nitrate. [The action consists in the assumption of the water by two equivalents, of the nitrous oxyd in one case, and of the nitrogen in the other. In the case of nitrous oxyd it may be represented thus $N_2O_2+H_4O_4=NO_6NH_4$; and in the case of nitrogen $N_2+H_4O_4=NO_4NH_4$]. If potash be present, the nitrite of potash will be produced by decomposition of the ammoniacal salt, and the ammonia set free may itself be nitrified. Without going into theoretical discussions, this hypothesis may be alluded to as one arrived at by legitimate analogies, and which it would be interesting and useful to test by experimental investigations."

The foregoing passage is exactly copied from the Report, with the exception of obvious typographical errors and the incorporation of the foot-notes enclosed in brackets.

As a further contribution to the history of the subject the following reference may be made: In the *Proceedings of the American Association for the Advancement of Science*, Fourth Meeting, held at New Haven, Conn., August, 1850, there will be found on page 206 a paper headed "*On a new test for nitrates.* By Prof. G. C. Schaeffer of Center Coll., Ky., read with comments by T. S. Hunt, Canada Geological Commission." This notice contains in a few lines an erroneous statement of the test, and in several more lines a criticism upon it, to which are appended brief replies of Mr. Hunt and Prof. Silliman, Jr., to this criticism. At page 403 however, there is a copy of the paper as sent *before* the time of meeting, and for the recovery of which, and its insertion, the writer is indebted to the kindnes of Mr. Hunt.

A few quotations from this paper must conclude this intrusion upon your patience.

"*New test for the Nitrites and Nitrates*, &c.—Chemistry has hitherto furnished no distinctive test for the nitrites when presented in small quantities. From the supposed unfrequent occurrence of these salts, the want of such a test has never been felt.

"For several years, I have been engaged in a research which has led me to believe that the nitrites are of far more frequent occurrence than is commonly supposed, and that they have been mistaken for nitrates, as the usual process, with pure sulphuric acid and protosulphate of iron, will

give the same reaction with both classes of salts." Among the difficulties encountered is named this one, "the nitrites are generally either destroyed or converted into nitrates with such readiness that it would be almost impossible to concentrate their solutions." After describing the care required in making this test, the following words are found: "With these precautions, I have found this test astonishingly delicate, in fact ranking with those for iron, iodine, &c. Using fused nitre, I have detected the presence of 1 pt. in 617,000 pts. of water; a bystander, wholly ignorant of the nature of the operation, pronouncing as to the color. Yet this salt contained about one-half its weight of undecomposed nitre."

Next follows a description of the conversion of this test into one for the nitrates, after which this remark is made: "In estimating the delicacy of this process, I had used pure rain-water, but before completing the experiments I was obliged to be absent for several days; on my return, I soon found that the water from the same cistern contained so much of nitrates and nitrites that it could no longer be used." "The interval had been marked by the occurrence of frequent and severe thunder showers."

It is very doubtful whether an earlier notice than this, of the presence of nitrites in rain-water, can be produced.

In spite of its unfortunate position in the volume, it seems that the paper just quoted has been republished in this country, in England and on the continent, and, as the much valued *Reports of the Smithsonian Institution* are widely distributed, the verification of the quotations above cited can readily be made.

It is a matter of regret that the want of time and the absence of documentary evidence, soon to be supplied, prevents, at present, the continuation of my contributions to the history of this subject as connected with chemical science; since the result, it is believed, would show another and an earlier origin for these views than any which has yet been assigned to them.

SCIENTIFIC INTELLIGENCE.

I. PHYSICS.

1. *On the Fraunhofer lines visible in the Solar Spectrum.*—ANGSTRÖM has communicated an interesting paper on Fraunhofer's lines, from which we shall extract a few notices relating to particular points not specially mentioned in the more recently published memoir of Kirchhoff. The author begins by referring to a previous memoir of his own, in which he had endeavored to show that a body in a state of glowing heat emits just the same kinds of light and heat which it absorbs under the same circumstances. The conclusions there arrived at were as follows.

The electrical spectrum is composed of two superposed spectra, the one belonging to the metal of the electrodes, the other to the gas through which the spark passes, the two spectra being distinguishable by the difference in appearance. Metallic compounds and metallic sulphids have in their luminous spectra the same lines as each of the bodies which the compound contains, and this affords a ready method of qualitative

analysis. In the same paper, the author remarked that Fraunhofer's lines were an inversion of the bright lines in the electrical spectrum, and that an explanation of the lines in the one system would probably furnish an explanation of those of the other, according to the principle laid down in that dissertation. In the present memoir, the author applies the general principle to the case of the sun's atmosphere, and distinctly states that, by determining coincidences between the dark lines in the sun's light and the bright lines in the electric spectra of different metals, we can determine what metals exist in the solar envelope. As Angström's memoir was read before the Royal Academy of Science at Stockholm, Oct. 8, 1861, it appears that the priority of conception, at least, belongs to him, while Kirchhoff has the honor of being the first to demonstrate the truth by direct experiment. Kirchhoff's map of the spectrum extends only from D to a little beyond F; the following statements will therefore interest the chemist. Between G and H there are fifteen strong iron lines, all having their counterparts in the solar spectrum. The two strongest of these lie at about one-fourth and three-fourths of the distance between H and G, and that nearest G is double and contains a calcium line. The third of the lines marked *b*, reckoning towards F, is double, and belongs to both magnesium and iron. Calcium has three strong lines at the violet end of the spectrum, of which two correspond to the H lines, and the third in order forms with one of the iron lines the abovementioned strong double line. Calcium has also six lines coinciding with lines of the G group, three between G and F, and groups of fine lines at E and between E and G. Aluminum exhibits two strong lines between the two H lines of the solar spectrum and corresponding to two dark lines. Between H and G aluminum probably forms a continuous spectrum. The manganese spectrum exhibits a considerable number of lines. Between G and H two groups of manganese lines coincide with two similar groups of iron lines, and between G and F there are also thirteen manganese lines closely approaching those of iron, and certainly corresponding to dark lines in the solar spectrum. Strontium has two strong lines between H and G which appear to correspond to solar lines, but the strong blue strontium line between G and F has no corresponding line in the solar spectrum. The author assumes further that the line C belongs to hydrogen. An interesting discussion of the theory of thermometric heat concludes Angström's memoir, which, it must be remembered, although first published in English in July, 1862, in reality preceded the important memoir of Kirchhoff.—*L., E. and D. Phil. Mag.*, xxiv, 1, July, 1862. W. G.

2. *On the violet flame of many Chlorids.*—GLADSTONE has observed that the majority of the lines in A. Mitscherlich's diagram of the flame of chlorid of copper (given by Miller in 1845) are common to a large number of chlorids if they are sufficiently heated. Thus, in the burning of old ship timber, which of course contains various alkaline and earthy chlorids, the light consists of three groups of lines, the first green and extending to *b*, the second bluish green and blue on either side of F, the third violet from midway between F and G to a little beyond G. These bands may be resolved by the spectroscope into groups of lines which are identical with the lines of chlorid of copper, as shown by ac-

tual angular measurement. The yellow sodium light is wanting. The chlorids of copper, platinum, gold, mercury, nickel, cobalt, zinc, iron, sodium, potassium, and barium, all exhibit the characteristic violet light when sufficiently heated. The explanation of these phenomena is not apparent in the present state of our knowledge.—*L. and E. Phil. Mag.*, xxiv, 417, December, 1862. W. G.

3. *On the Solar Spectrum.*—MERZ has communicated a few notes on the construction of the spectroscope and kindred subjects, which are worthy of attention. The author in the first place gives a *resumé* of the results of Fraunhofer with the spectra of fixed stars, and then quotes very briefly from a memoir of Prof. Donati of Florence, which we have not seen, and which describes the spectra of Sirius, Vega, Procyon, Regulus, Fomalhaut, Castor, Atair, Capra, Arcturus, Pollux, Aldebaran, Rigel, and Antares. From these observations, it appears probable, according to Merz, that iron plays the chief part in the atmosphere of all these stars. The next notice refers to the use of very large prisms and telescopes for the observation of the spectrum. The author found that a flint glass prism of 60°, with a face 43 lines in breadth, placed in the corresponding section of a beam of parallel rays emerging from a condenser with an aperture of 34 lines, resolved the line D into five lines. A prism with a face of 19 lines resolved D into three lines. When eleven prisms were used, with an angle of about 480°, D was resolved into seven lines. The author expects that great advantage will result from an increase in the size of the prisms and telescopes, and proposes to experiment in that direction.—*Pogg. Ann.*, cxvii, 654, Dec. 1862.

W. G.

[Compare Prof. Rood's article on Merz's results, p. 356 this volume.—*Eds.*]

4. *A new Spectroscope.*—LITTROW has devised a new form of spectroscope, in which only one telescope is necessary, and in which four prisms are made to give the same dispersion as eight with the old arrangement. In this instrument the bundle of rays which diverge from the slit are rendered parallel in the usual manner by an achromatic lens placed at the opposite end of the condensing telescope. The rays then traverse successively four or more flint glass prisms of 60°, then fall upon a mirror which reflects them back so that they again traverse the prisms and fall upon the lens of the condenser. This lens produces an image of the spectrum near the slit, where a prism is placed so as to throw the rays into a lateral ocular. The prisms are so connected that by turning a pinion the whole spectrum can be brought in succession into the field of view, each prism being constantly in the position of minimum deviation for each ray. The apparatus with four prisms costs in Vienna only 420 francs, and gives, according to the inventor, many lines not seen in Kirchhoff's map. An apparatus involving a similar principle has also been described, though very obscurely, by Janssen.[1] The adjustment of the prisms in Littrow's apparatus ought to be adopted in all spectroscopes in which more than one prism is employed.—*Cosmos*, xxi, 650.

W. G.

[1] Comptes Rendus, June 23d, 1862.

5. *On the Spectra of the Alkaline Metals.*—Wolf and Diacon have studied the spectra of the alkaline metals produced by very high temperatures, and have found that even the light of sodium is not monochromatic. The authors obtained their spectra by passing hydrogen through a slightly bent tube containing potassium, sodium, or a volatile chlorid, and heated to a convenient temperature. The hydrogen, on issuing from the tube, is mixed with oxygen and burned. Under these circumstances, many metallic chlorids give remarkably well defined and long-continuing spectra. Metallic sodium gives in this manner six well-defined lines between C and F, upon a faintly colored ground, which, however, is not continuous, but exhibits two sudden variations in intensity. Potassium gives a magnificent spectrum, in which eleven lines—for the most part already noticed by Debray and Grandeau—are observed. Chlorid of lithium gives four brilliant and characteristic lines. Finally, the chlorids of copper and zinc give very well defined spectra of great intensity.—*Comptes Rendus*, lv, 334. W. G.

6. *Contributions to spectral analysis.*—Böttger has published a few notices relating to the spectral analysis, from which we extract what is new. Selenium gives, according to Böttger, between the yellow and the violet, a very large number of equidistant dark lines. Native selenid of mercury gives the same reaction. When coal-gas, before reaching a Bunsen's burner, is caused to pass through a wash-bottle containing a little chloroform, a flame is obtained which exhibits a beautiful green inner cone, the spectrum of which exhibits two dark blue lines at the extreme end of the violet, three very broad green lines between D and *b*, and a broad blue line between F and G. Chlorid of bismuth produces a great number of bright lines in the red and blue, which, however, last but an instant. Fluor spar gives, in addition to the calcium lines, a beautiful clear blue line, which, according to Böttger, is characteristic of fluorine. Böttger found this line in all the varieties of fluor spar as well as in chemically pure fluorid of calcium, but not in cryolite or fluorid of potassium. The spectrum of cyanogen—long since observed by Draper—is of extraordinary beauty.—*Journal für Prakt. Chemie*, lxxxv, 392. W. G.

7. *On the spectrum of Sodium.*—Fizeau has made the very noteworthy observation that metallic sodium in a state of active combustion gives a *continuous* spectrum in which the line D appears as a *dark* line. Potassium and magnesium do not give continuous spectra under the same circumstances, and Fizeau's observation at present stands entirely isolated, without even an attempt at explanation.—*Comptes Rendus*, liv. W. G.

[Draper's experiments have shown that metals up to a white heat give continuous spectra. At a higher temperature each metal appears to give a discontinuous spectrum or one marked by brilliant lines with intervening dark spaces, as shown by Kirchhoff. Is it not possible that at a still higher temperature the spectra again become continuous, the temperatures at which this takes place being different for each substance? This theory explains the occurrence of more brilliant lines at very high than at lower temperatures, continuity being simply the limit finally reached. If we admit the correctness of this view, Fizeau's experiment may be explained very simply, since the intense light of the body of the flame

of burning sodium, which in itself gives a continuous spectrum, passing through the portion of the flame ignited at the edges in contact with the air and of lower temperature, or still more probably through vaporized metallic sodium which has escaped combustion, will reverse the brilliant line D, and thus give a dark line upon a continuous bright spectrum. If this explanation, upon further knowledge of the facts, should prove correct, it will not be necessary, with Kirchhoff, to suppose that the solid body of the sun is ignited or luminous. For the temperature of the photosphere may reasonably be supposed to be highest nearest the surface of the body of the sun, since there the condensation is greatest. Those layers or strata nearest the sun will then give continuous spectra, and the rays from there passing through the outer strata will give spectra containing Fraunhofer's lines, according to the principle laid down by Angström, Kirchhoff, and others. The temperature of burning sodium cannot be determined by calculation, so long as we are ignorant of the specific heat of the oxyd NaO in the form of vapor. We may assume that this specific heat under a constant pressure is not more than one-half of that of water, and we shall obtain the temperature of combustion by dividing the heat of combustion by the specific heat. According to Favre and Silbermann, we have for the heat of combustion the number 3195, which, divided by 0·5, gives for the temperature of combustion 6390° C., which is less than that of hydrogen (=8061° C.) burning also under a constant pressure. But if the specific heat of soda, NaO, in the form of vapor, be taken as one-fourth of that of water, we shall have for the temperature of combustion 12780° C. It appears by no means improbable that the actual temperature is even higher than this.—W. G.]

8. *On the indices of refraction of fluid homologous compounds.*—Landolt has given a very interesting and valuable investigation of the coefficients and indices of refraction and dispersion of the acids of the homologous series $C_{2n}H_{2n}O_4$, of which formic acid is the first term. The author employed a Meyerstein's spectrometer, reading to 10 seconds of arc. The liquids were enclosed in hollow prisms, the refracting angles of which were carefully measured after each cleaning and remounting. The source of light employed was a Geissler's hydrogen tube placed immediately in front of the slit of the spectrometer. The passage of the discharge of a Ruhmkorff's coil gave the three brilliant hydrogen lines α, β, γ, observed and described by Plücker, and for which that physicist found the wave lengths $\lambda_\alpha = 6{\cdot}533$, $\lambda_\beta = 4{\cdot}843$, $\lambda_\gamma = 4{\cdot}339$. The position of these lines is fixed by the wave lengths of Fraunhofer's lines, which are as follows:

λ_B.	λ_C.	λ_D.	λ_E.	λ_F.	λ_G.	λ_H.
6·878	6·564	5·888	5·260	4·843	4·291	3·928

expressed in hundred-thousandths of a centimeter. The employment of the three hydrogen lines, α, β, γ, permits the observations to be made at all times and with the greatest facility and accuracy. Landolt further determined the indices of each substance for a series of temperatures, selecting 20° C. as the normal temperature. The bulb of the thermometer was in each case plunged directly into the liquid; the prism and liquid were heated to 30° C. and allowed to cool slowly, the observations being made from degree to degree. The method of measurement em-

ployed was that of least deviation. According to Cauchy, the connection between the wave length and corresponding index of refraction is expressed by the equation

$$\mu = A + \frac{B}{\lambda_2},$$

in which A is the coefficient of refraction, and B the coefficient of dispersion. If the indices μ_α and μ_γ be determined by direct measurement for a given substance, we have the two equations

$$\mu_\alpha = A + \frac{B}{\lambda_\alpha^2}$$

$$\mu_\gamma = A + \frac{B}{\lambda_\gamma^2},$$

from which we find

$$B = \frac{\mu_\gamma - \mu_\alpha}{\frac{1}{\lambda_\gamma^2} - \frac{1}{\lambda_\alpha^2}} \qquad A = \mu_\gamma - \frac{B}{\lambda_\gamma^2}.$$

Thus for water the indices found were

$$\mu_\alpha = 1{\cdot}33120$$
$$\mu_\beta = 1{\cdot}33723$$
$$\mu_\gamma = 1{\cdot}34050.$$

From μ_α and μ_γ and the wave lengths

$$\lambda_\alpha = 6{\cdot}533$$
$$\lambda_\gamma = 4{\cdot}339,$$

we obtain for the constants A and B at 19° C. the values

$$A = 1{\cdot}32386$$
$$B = 0{\cdot}31328.$$

Calculating from these the value of μ_β, we find $\mu_\beta = 1{\cdot}33722$, which agrees very closely with the actual measurement, 1·33723. For the line D the calculated index was 1·33290, the value found by direct measurement also 1·33290. The author gives the values of A and B for each substance at each temperature measured. With these values he also calculates the indices of refraction of each substance at the normal temperature of 20° C. for the seven Fraunhofer's lines B . . . H. The most interesting general results of Landolt's measurements, which begin with formic and end with œnanthylic acid, are as follows. The indices of refraction increase with the number of equivalents of carbon and hydrogen, but by no means uniformly. The indices for all the acids increase in about the same degree as the wave lengths diminish. The curves for the different acids are not equidistant, but, excepting in the case of formic acid, are nearly parallel. The coefficient A also increases irregularly with the carbon and hydrogen. The diminution in A for 1° C. becomes less from acetic acid upward, but the differences are very small. The change in the case of formic acid is less than in any of the others. The coefficient B also increases with the increase of carbon and hydrogen, except in the case of formic acid. Also the elongation of the spec-

trum, as measured by the difference $\mu_\gamma - \mu_a$, increases with the degree of the acid in the series, excepting in the case of formic acid. The author promises a further discussion of the results of his measurements, as well as an examination of the indices of the homologous alcohols $C_{2n}H_{2n+2}+O_2$.—*Pogg. Ann.*, cxviii, 353. W. G.

II. CHEMISTRY.

1. *On the coloring matters derived from aniline.*—Dr. HOFMANN has published an elegant investigation of the colors derived from anilin, which places the chemical nature of these substances in a clear point of view. Hofmann finds that the red coloring matter, produced by the action of the chlorids of carbon, tin, mercury, and other metals, and of certain oxydizing agents upon anilin, is an organic base which has the formula $C_{40}H_{19}N_3$. This base he terms Rosanilin; in a pure state it is a perfectly colorless crystalline body, slightly soluble in water, and becoming red on exposure to the air. It dissolves in alcohol with a dark red color. The change of color is not accompanied by a change of weight. On distillation, the base yields anilin and a carbonaceous mass. The hydrate is $C_{40}H_{19}N_3, 2HO$. The base is triacid, but forms three classes of salts:

$$C_{40}H_{19}N_3.HCl$$
$$C_{40}H_{19}N_3.2HCl$$
$$C_{40}H_{19}N_3.3HCl.$$

The salts with one equivalent of acid are very stable; they exhibit for the most part a green metallic lustre like the wings of cantharides. They are red by transmitted light, and their solutions have a magnificent red color. The salts with three equivalents of acids are yellowish-brown, both in the mass and in solution. The chlorids $C_{40}H_{19}N_3.HCl$ and $C_{40}H_{19}N_3.3HCl$ unite with bichlorid of platinum to form uncrystallized salts; the triacid chlorid loses acid on heating to 100° C., and becomes indigo-blue, which Hofmann attributes to the formation of an unstable intermediate chlorid. The author describes several crystallized salts of rosanilin; the acetate $C_{40}H_{19}N_3.C_4H_4O_4$ is the most beautiful. The action of nascent hydrogen converts rosanilin into leucanilin, $C_{40}H_{21}N_3$, which is colorless and crystalline, and forms salts containing three equivalents of acid. Oxydizing agents convert leucanilin into rosanilin, so that Hofmann compares the two bases to blue and white indigo.

Hofmann has further examined a beautiful yellow coloring matter which is formed in the oxydation of anilin, and which he terms chrysanilin. This base exists in large quantity in the resinous substance which accompanies rosanilin in all the usual modes of preparation. The base in question appears to have been first prepared by Nicholson, and presents a fine yellow amorphous powder, which is scarcely soluble in water but very soluble in alcohol and ether. The formula of chrysanilin is $C_{40}H_{17}N_3$, and it yields two classes of well crystallized salts, being monacid and biacid. The most remarkable property of this base is the formation of a nitrate so insoluble that chrysanilin is the best known reagent for nitric acid. One gramme of nitrate of potash in one litre of water immediately gives a crystalline precipitate with a solution of chrys-

anilin. The formulas of the three bases described by Hofmann exhibit a remarkable connection—a sort of homology in which H_2 is the constant difference. Thus we have

Chrysanilin, - - - $C_{40}H_{17}N_3$
Rosanilin, - - - - $C_{40}H_{19}N_3$
Leucanilin, - - - $C_{40}H_{21}N_3$

The conversion of chrysanilin into rosanilin and leucanilin appears possible, but has not yet been effected.

Dr. Hofmann has also examined the beautiful blue coloring matter obtained from crude chinolin by the action of the iodids of methyl, ethyl, &c., and termed cyanin. The iodid of this base has the formula $C_{60}H_{39}N_2I$. Another base homologous with this is found in the commercial cyanin. Its formula is $C_{56}H_{35}N_2I$, and it appears to be derived from pure chinolin, $C_{18}H_7N$, while the first mentioned base is derived from lepidin, $C_{20}H_9N$. The author distinguishes two phases in this reaction. In the first we have the equation

$$C_{20}H_9N+C_{10}H_{11}I=C_{30}H_{20}NI;$$

in the second we have

$$2C_{30}H_{20}NI+KO,HO=C_{60}H_{39}N_2I+KI+2HO.$$

—*Comptes Rendus*, liv, 428, lv, 817, 849. W. G.

ANALYTICAL CHEMISTRY.

2. *On the Analysis of Borates and Fluoborates.*—In solutions which contain only boric acid and alkalies, MARIGNAC (*Fresenius's Zeitschrift für Analytische Chemie, drittes Heft,*) determines the former as follows: The solution is neutralized with chlorhydric acid and chlorid of magnesium, or better, chlorid of magnesium-ammonium is added in such quantity that to one part of boric acid at least two parts of magnesia are present. The liquid is now made ammoniacal and finally is evaporated to dryness in a weighed platinum vessel. Should the addition of ammonia cause a precipitate which does not readily vanish on warming, sal-ammoniac must be put in until the liquid becomes clear. During the evaporation, it is well to add a few drops of ammonia from time to time. When the mass is dry it is heated to redness, then treated with boiling water, the residue is collected on a filter and washed with hot water until the washings are not in the slightest affected by nitrate of silver.

This first residue contains together with excess of magnesia the larger part of the boric acid. A small amount of the latter always goes into solution. The filtrate and washings are treated with ammonia and again evaporated, ignited, and washed as before. The second filtrate and washings are once more treated in the same manner, when great accuracy is required.

The three residues are ignited together in an open crucible as strongly as possible and so long as to decompose the traces of chlorid of magnesium which they may contain. When they are weighed, it only remains to estimate the magnesia in them, to learn by difference the quantity of boric acid. This can be done either by dissolving in an acid and precipitating ammonia-magnesian phosphate or more rapidly by dissolving in a known volume of standard sulphuric acid at a boiling heat and determining the excess, with help of an alkali solution.

Should an insoluble, heavy, gray residue remain on treating with acid, it must be collected and its weight deducted from that of the borate of magnesia. It is platinum.

The subjoined example illustrates the method and demonstrates that alkali-chlorids in large excess have no serious influence on its accuracy. 0·764 grm. of pure borax, containing 0·280 grm. of boric acid were dissolved with 2 grm. of chlorid of sodium and 3·2 grm. of crystallized chlorid of ammonium-magnesium were added.

First residue	= 0·5720	contained boric acid	= 0·2667 grm.
contained magnesia	= 0·3053		
Second residue	= 0·1040	" " "	= 0·0093 "
contained magnesia	= 0·0947		
Third residue	= 0·0645	" " "	= 0·0020 "
contained magnesia	= 0·0625		
		Total,	0·2780

Other determinations gave results of equal accuracy. From insoluble compounds the boric acid is obtained in solution by fusing with thrice their weight of carbonate of soda and exhausting the mass with water. In case of silicates the alkaline solution is digested with chlorid of ammonium to precipitate silica.

When one operates with a fluo-borate the solution of the carbonate of soda fusion is digested with sal-ammoniac to decompose a good share *but not all* the soda-carbonate, and thereupon is precipitated with a neutral or ammoniacal solution of chlorid of calcium. The precipitate of Ca Fl and $CaO\ CO_2$ is washed,—a matter easily accomplished—dried, gently ignited, treated with acetic acid, evaporated to dryness and the pure Ca Fl collected, washed and weighed. The filtrate, after removing lime by carbonate and a few drops of oxalate of ammonia, may be treated as before described for the estimation of boric acid. From a mixture of 2·420 grm. of fluor-spar with 0·382 grm. of borax containing 0·190 grm. of fluorine and 0·140 grm. of boric acid, were obtained by this process (except that no fusion was made), 0·1883 grm. of fluorine and 0·1362 grm. of boric acid. In the analysis of borofluorid of potassium a loss of fluorine equal to 1·5 to 1·8 per cent occurred which Marignac thinks might have been avoided by employing a caustic alkali in the fusion. S. W. J.

PHOTOGRAPHY.—

3. *Collodion.*—We translate from *La Moniteur de la Photographie* for February 15th, 1863, the following letter addressed to the editor by A. JEANRENAUD. Mr. Jeanrenaud is a well-known skillful amateur photographer.

"*Mr. Editor*:—If you and several other gentlemen had not requested of me the formula for the collodion I use, I should perhaps never have determined to publish it. In general, each photographer has his own special processes, so that it may be said that there are as many formulas for collodion as there are operators; but, since you judge, from the results I have obtained, that it will be useful to make my formula known to your readers, I do it with the more pleasure, as I hope that those who shall take the pains to try it will have no reason to

regret it. This formula is less empirical than it seems, for it is the result of a long series of researches and trials, concerning which it would be useless to dilate. Such as it is, it is *good*, and has given me for several years very constant results; and, I may add, that unlike other collodions, time has upon it no other influence than to improve it, which has determined me always to have a supply a year old on hand.

Formula.

For one litre of collodion,		=	35¼ fluid ounces.
Ether at 62°,	800 grammes,		28½ " "
Alcohol at 40°,	250 "		8·8 " "
Very soluble gun cotton,	8 "		123 grains.
Iodid of cadmium,	9 "		139 "

Upon complete, solution twenty-five drops of pure bromine are added. The color becomes very intense, for there is some iodine set free, and a consequent formation of bromid of cadmium. From this litre I extract 100 grammes—one tenth part of the whole quantity—which I place in a separate flask. Into this 100 grammes are dropped twelve or thirteen drops of highly concentrated liquid ammonia. A very thick golden-yellow precipitate is formed, so thick that it will not mingle with the supernatant liquid even by vigorously shaking the flask. It is not easy to define with chemical exactness the constitution of this precipitate; but what is certain is, that it suffices to add to it a few drops of crystallizable acetic acid to dissolve it and render the collodion perfectly limpid. This last operation with acetic acid is somewhat uncertain, as the quantity varies according to the quality of the alcohol and ammonia used. I now pour back into the first flask the 100 grammes upon which I have just operated, and let the whole stand for fifteen days before using it. During this time the collodion, however red it may be, changes gradually until it attains at last a pale straw-color, which tint it ought to keep. If the collodion is found to be insufficiently iodized (although the proportions above given ought to be quite sufficient), I ought to say that it would not be proper to add the iodid of cadmium directly, or the collodion will become cloudy and cannot be cleared by filtration. It is necessary to dissolve the iodid first in a small quantity of collodion separately, and mix afterwards. In conclusion, the collodion contains iodids, bromids and acetates. It may happen, and it does happen in fact, that it forms in the negative bath small crystals of acetate of silver; I have never had any reason to complain of this, on the contrary I think it is to the reaction which produces them that we must attribute the good qualities of this collodion."

E. E.

III. METALLURGY.

1. *Thallium in furnace products;* by W. T. Roepper, (communicated in a letter to Prof. Geo. J. Brush).—In examining before the spectroscope some of the dust deposited by the tunnel-head flame on the boilers of the Bethlehem Iron Works, a sharp, bright green line flashed up midway between the green calcium and Ba α line, which, judging from Crookes' description, appeared to be the thallium line, a conjecture which Prof. Brush verified by comparison with some of Lamy's chlorid of thallium.

The line appeared at 90° on the scale of the instrument, sodium being at 60° and the red and violet potassium lines respectively at 7° and 226°. In addition to the above lines, the spectra of sodium, potassium, lithium and calcium are distinctly displayed by this dust. Thallium and the alkalies seem to exist in the dust, at least in part, as sulphates, which can be extracted by water. Similar dust from one of the other furnaces along the Lehigh gave the same results; hence it is not unlikely that thallium is a common product of the anthracite furnaces, and is perhaps derived from the pyrites accompanying the coal, though Crookes found the non-cupriferous pyrites to be almost free from this element.[1]

It is only the dust which remains lying on the boilers, or is deposited on the iron doors or shutters of the boiler-chamber, which gives the thallium reaction, while that which has fallen to the floor does not show it. The reason probably is, that it is volatilized by the higher heat of the flame, and escapes through the chimney as soon as it is removed from the comparatively lower temperature of the steam and the outer doors, which seem to act as condensers. I have not been able to detect it in the ashes of anthracite from a common stove, while they beautifully display the sodium, potassium and lithium lines.

Bethlehem, Pa., April 8, 1863.

2. *Bessemer's process for the production of Iron and Steel.*—This method for converting the purer varieties of pig-iron into steel and bar-iron is constantly increasing in favor among European ironmasters.

In a recent communication to the "*Berggeist*," Prof. TUNNER states that thousands of cwt. of Bessemer-steel and iron are now annually produced in England and Sweden; that Bessemer-steel is already an article of commerce in Germany; and that large works are also being erected for the employment of this method in France.

Whenever the proper raw-material is used, Bessemer's process gives steel which in all respects is fully equal to the best varieties of cast-steel; and iron of as good quality as the best forge-iron. The loss in converting pig-iron into steel, by this method, is 12 to 15 pr. ct., and in making bar-iron 18 to 22 pr. ct. In 5 to 10 minutes, 15 to 20 cwt. of fluid pig-iron are converted into steel or bar-iron with scarcely any cost for fuel, and without hand labor. The pressure of blast used is from $\frac{1}{2}$ to $1\frac{1}{2}$ atmospheres, and the amount is 800 to 1200 cubic feet of cold air of the ordinary atmospheric density.

Only good charcoal-iron is adapted for conversion by this method, and the reason of the failure of the earlier experiments was the employment of improper and inferior raw-material. Swedish pig-iron is now always used in England for the production of the best sorts of steel and iron. In some of the new iron works attempts have been made to improve the quality of English pig-iron which has been carried to the point of conversion, by adding to it melted Swedish pig-iron; manganese compounds have also been used for the same purpose. But the separation of the

[1] Mr. Crookes has recently announced that he has found thallium in comparatively large quantities in the deposit from the flues of Mr. Spence's pyrites burners at Manchester.—*Chem. News*, vii, 150.—G. J. B.

deleterious substances associated with carbon in pig-iron still remains an unsolved problem. For the success of this method, a good quality of pig-iron is therefore indispensable, and further a high temperature; this last is attained by converting large quantities of iron in a single operation. In Sweden 15 cwt. for a charge is the minimum quantity used, and if 60 to 100 cwt. be employed the result would be still more favorable. In converting large quantities at one operation, the cost is proportionally diminished, and the product may also be made more uniform.

One great advantage of Bessemer's process is that so much larger quantities of material can be operated upon at one charge than in the ordinary methods of refining, and this quantity is not restricted within narrow limits as in puddling and hearth refining. For the production of the proper temperature, the relative amount of blast to the pig-iron operated upon should be carefully regulated. If too little, the process goes on slowly, and much heat is lost by radiation; on the other hand, if too much blast is used, there is also a loss from the heat carried off by the air which is forced through the iron before it has effected the desired decomposition. The pressure of the blast must, at all events, be greater than that of the column of iron in the furnace, in order that the bath of molten iron shall be thoroughly penetrated and the whole melted mass set in violent agitation. In Sweden the pressure of half an atmosphere has in most cases been found sufficient, while in England a pressure equal to $1\frac{1}{2}$ atmospheres has been used.

Tunner places particular emphasis on the employment of a high pressure with hot blast. He says that if the blast were to be heated to 200–300° C., or perhaps even to 500–600° C., the conversion would unquestionably proceed with great regularity and completeness, and the difficulties in the manufacture of soft bar-iron and steel would be overcome. Further, it is to be borne in mind, that, in order to produce a given variety of steel or iron, the process of conversion must be interrupted whenever the refining has reached the desired point; this last is determined by observing the character of the gases and sparks which escape from the furnace, very much as is the case in hearth refining: practice is of course required to be able to determine this point with accuracy. The fracture of the metal serves as a control in sorting the different qualities. The cost for furnace repairs is much less than was at first anticipated, but the waste product in conversion (equal to 20–30 pr. ct. when the iron is made into bars) demands consideration, especially as no use has yet been found for this more or less impure product. If, however, we take into consideration the length of time that has been necessary to bring the puddling process to its present perfection, while on the other hand Bessemer's process has accomplished so much in so short a time, we have every reason to hope that the day is not far distant when the still remaining difficulties in this process will be reduced to a minimum.—*Polytechnisches Journal*, clxvi, 447. [A wide field is open for the application of Bessemer's process in this country, where pure iron ores, fully equal in quality to those of Sweden and Norway, occur in such abundance.—G. J. B.]

IV. AGRICULTURAL CHEMISTRY.

1. *Atmospheric Nitrite of Ammonia and its Origin.*—E. Bohlig describes (*Ann. der Ch. u. Ph.*, cxxv, 21–33) the results of long study of this subject, made at the same time as, but independently of, the investigations of Böttger and Schönbein.

Barral found in the rain water collected at the Paris Observatory, during the year ending June, 1852, an average of 151·81 grms. of nitric acid and 41·82 grms. of ammonia per cubic metre. These results have not been received with confidence on account of the deficiencies of the methods at Barral's disposal for the estimation of nitric acid. Boussingault, Bineau, Lawes & Gilbert, and Way found, on the contrary, in all atmospheric waters an excess of ammonia above that required to form nitrate with the nitric acid present, and hence it has been assumed that there exist both nitrate and bicarbonate of ammonia in the atmosphere. Bohlig gives as the result of his researches: 1st, that "normal atmospheric air and normal rain-water never contain bicarbonate of ammonia, but do contain nitrite of ammonia." 2d. "The nitrite of ammonia originates wherever ozone comes in contact with nitrogen, as well as in all cases of combustion in free air." The experiments on which the first of these conclusions is based are purely qualitative in character. According to Bohlig the most sensitive reagent for free ammonia and carbonate of ammonia is chlorid of mercury—a solution containing but $\frac{1}{2000000}$ of ammonia giving with this salt a perceptible white turbidity. If 40 cc. of water exempt from ammonia (such is the water of many springs, but distilled water rarely,) is mixed with 5 drops of a solution of corrosive sublimate (1 of salt to 30 of water) and the same quantity of a solution of the purest carbonate of potash (1 of salt to 50 of water), the whole remains perfectly clear for days together in closed vessels. If the solutions are much more concentrated, oxyd of mercury will separate. If water containing ammonia in combination with the stronger acids be tested with corrosive sublimate and carbonate of potash, the same reaction—turbidity from separation of amido-mercuric chlorid—takes place as happens with carbonate of ammonia and sublimate alone, double decomposition occurring between the ammonia salt and carbonate of potash.

Bohlig found, testing the water of each considerable rain for a year, that in no case was any turbidity produced by sublimate alone, while sublimate and carbonate of potash together in all cases gave a turbidity or even a precipitate. Contrary to the statements of the text-books, Bohlig also found that the first portions of the distillate from rain water were exempt from ammonia. Bohlig thus concludes that the ammonia or carbonate of ammonia found in any case in distilling rain water must have come from admixture of carbonate of lime with the water.

In opposition to these results we have the statement of Boussingault that the water of fogs, in most cases, has a distinct and in some a strong alkaline reaction, (*Agronomie*, ii, 227–8.) Besides it is not easy to understand how the methods employed by Boussingault and Way in the determination of nitric acid should not really have given the amount

of nitrous acid (calculated as nitric acid). The former first added carbonate of potash to a litre of the water, then slowly evaporated to a few cubic centimeters and finally estimated the oxydizing effect of the supposed nitric acid by indigo, in connection with strong chlorhydric acid. (*Agronomie*, ii, 299). Way made a pint of the water alkaline with lime, and boiled it down in a long-necked flask to a small bulk, filtered from carbonate of lime, and evaporated in another flask to dryness at a final heat of 350° F. This residue was acted upon by iodid of silver and chlorhydric acid, and the liberated iodine was measured by sulphurous acid. (*Jour. Roy. Ag. Soc. of England*, 1856, xvii, 157.)

Bohlig, in the paper under notice, says, that rain water or a solution of nitrite of ammonia may be boiled a short time, and evaporated to one-third at a gentler heat, without loss of nitrite. The nitrites of lime and potash, in presence of excess of base, are supposed, on all hands, to be fixed at ordinary temperatures. We should expect then that a water containing nitrite of ammonia, when evaporated with carbonate of potash or lime water, would lose carbonate of ammonia, or free ammonia, while the nitrous acid would be completely retained in the residue.

According to Bohlig, nitrite of ammonia in dilute solution, though suffering no chemical change by concentrating to a certain degree, is completely decomposed by further evaporation at or near 212° F. On the other hand, nitrate of ammonia may be boiled to dryness without loss. As, now, rain water always acquires a blue color after a longer or shorter time with acidulated iodid of potassium-starch-paste, while the residue of its evaporation to dryness does not give this reaction, Bohlig concludes that nitrite of ammonia is, while nitrate of ammonia is not, a normal ingredient of atmospheric waters.

From this result, it was warrantable to infer that the atmosphere itself contains no nitrate but only nitrite of ammonia. To examine the atmosphere more directly, Bohlig passed 20 cubic feet of air slowly through a small quantity of pure water. By the above mentioned tests he found no carbonate but some more fixed salt of ammonia, though he could not obtain any reaction for its acid. In imitation of rain, he now caused pure water to fall in a very slender stream upon a long slip of purified paper which hung in the free air and conveyed the water drop by drop into a capsule placed beneath. The water which had thus exposed an enormous surface to the atmosphere contained both ammonia and nitrous acid in such quantity as readily admitted of detection, and the results of numerous trials in this way made in various conditions of the atmosphere, when the sky was clear and when cloudy, after thunder storms and after long gentle rains, were always *qualitatively* the same.

Bohlig does not admit that nitrite of ammonia is formed by the direct union of nitrogen and water in the act of evaporation, as Schönbein believes; but is of the opinion that this compound is simply collected by water from the atmosphere, where it previously existed.

This explanation he conceives to account for the following fact, viz: when spring water that is free from ammonia salts is distilled, the first portions that pass over always render very slightly turbid the mixture of mercuric chlorid and carbonate of potash. When the distillation becomes rapid, this reaction disappears, but recurs again if the fire be slackened

and then again urged. Here, the air, that occupies the helm of the still, yields, according to Bohlig, its nitrite of ammonia to the condensing vapors.

Our author says further that the statement of Schönbein, that water and nitrogen unite directly to form nitrite of ammonia, appears doubtful, because the evaporation-experiments of the latter were made with unlimited quantities of air, and no account was taken of the preëxistence in it of the nitrite. But the nitrite always occurs in the atmosphere, though in proportions that vary extraordinarily with meteorological conditions.

In the experiment just described, with the slip of paper, Bohlig often observed that the water evaporated to less than one-sixth its original bulk before giving any reaction for nitrous acid. This happened, for example, after a protracted rain. At other times, when the weather was fine, the water gave the reaction of nitrous acid after once flowing over the paper, even when its quantity was scarcely diminished by evaporation. Schönbein often failed to obtain nitrite, in his own experiments.

Further trials which militate against Schönbein's theory are the following: 50 cc. of pure water were distilled in a rapid stream of air (more than three cubic feet) made free from nitrite of ammonia by passing through oil of vitriol and a long potash tube. The temperature rose from 12° to 100° C. After cooling, the distillate gave no reaction either for ammonia or nitrous acid. This experiment was repeated in the same manner, save that a less quantity of unwashed air was passed through it. Both the distillate and the residue in the retort gave most decided reactions for an ammonia salt, though nitrous acid was not detectable, from the inferior delicacy of the iodine starch test.

Five grms. of carbonate of potash, free from nitrous acid and ammonia, were allowed to *deliquesce* in the air. The liquid thus obtained gave very evident reactions for nitrite of ammonia. From this experiment we might conclude, using Schönbein's logic, that water in the act of *condensation* in presence of nitrogen unites with the latter.

Bohlig describes another highly interesting experiment. Hydrogen, illuminating gas, and alcohol, respectively, were burned in a tall bell-glass connected by its tubulure with several Woulfe's bottles containing pure water, and finally with an aspirator. The combustible was thus consumed in a stream of common air. After several grammes of alcohol (or corresponding quantities of the gases mentioned) were burned, the water of the absorbing vessels was perceptibly *acid* in its reaction, and with the iodid-of-potassium-starch test struck at once a deep-blue color. Ammonia was present only in minute traces, most perceptibly in the water which condensed on the sides of the bell and collected in a capsule placed underneath. This result indicates that the small amount of nitrite of ammonia found in the acid liquid was not produced in the experiment by union of water-vapor and nitrogen, but was simply condensed from the atmosphere.

Bohlig promises further researches, in which the air that feeds the flame shall be first purified from nitrite of ammonia.

It is plain that this whole subject requires thorough experimental revision. The facts now in our possession are certainly not sufficient to

warrant the assumption that nitrite of ammonia is formed from nitrogen and water; while at the same time some of Schönbein's experiments are scarcely explainable on any other hypothesis. S. W. J.

2. *The Nitrogen Question.*—Liebig,[1] Nicklès,[2] and others regard the theory of Schönbein with enthusiastic favor, since in their view it relieves the "nitrogen question" of agricultural chemistry of all embarrassment, and demonstrates that the atmosphere is to plants a source as abundant as unfailing of combined or assimilable nitrogen.

We must emphatically dissent from any such conclusion, for two reasons: 1st. It is not *proved* that there is in the atmosphere more assimilable nitrogen than corresponds to what has already been determined, in a manner that we must at present regard as entirely trustworthy, by Way and Boussingault. Lawes and Gilbert, at Rothamstead, England, collected all the waters of rain, snow, dew and fog that could be gathered during the years 1855–6. Way analyzed these waters, and found in them, for 1855, 7·11 lbs. of ammonia and 2·98 lbs. of nitric (nitrous?) acid, for 1856, 9·53 lbs. of ammonia and 2·80 lbs. nitric acid—amounts corresponding to 6·63 and 8·31 lbs. of nitrogen, respectively, for an acre of surface. 2d. It is not *proved* that any nitrogen is made assimilable—converted into nitrite—by the act of evaporation. Until solid facts have been accumulated to a considerable extent, especially until quantitative investigations really demonstrate that combined nitrogen is much more abundant than appears from the researches of Way and Boussingault, we are not warranted in making such positive deductions from the results of Schönbein, interesting and valuable as they are. S. W. J.

V. MINERALOGY AND GEOLOGY.

1. *On the composition of Columbite.*—H. Rose considers this mineral to be essentially a compound of hypocolumbic acid with protoxyd of iron and manganese. It is, however, difficult to deduce a rational composition from many of the analyses of the columbite from Bodenmais and Connecticut. The specimens from these localities vary exceedingly in their densities, their powder varies in color, and by careful observation it can be seen that they are more or less decomposed. The crystals are not unfrequently traversed by rifts and seams, and, on being broken, the fractured surfaces are often found coated with a thin layer of impure hypocolumbic acid, which last can be easily separated from the mineral. It would appear that columbite is partially decomposed by the action of water and the atmosphere, and that a portion of the iron and manganese is removed as carbonate; another portion of the iron is converted into the magnetic oxyd, which gives the pulverized mineral a black color, while the color of the powder of the undecomposed mineral is cherry-red. The specimens having the highest specific gravity are those which are the most decomposed. The specimens of columbite from Greenland and from the Ilmen Mountains retain their original properties: they have a lower density than the specimens from Bodenmais and Connecticut, and their density is always constant; their powder is cherry-red, never black. It is only from the analysis of these unaltered specimens that we

[1] Chemistry applied to Agriculture, 7th edition.

[2] This Journal, [2], xxxv, 263.

can arrive at a correct interpretation of the true composition of this mineral.

Nine analyses of the columbite from Bodenmais have been made in Rose's laboratory by Rose, Afdéef, Jacobson, Chandler, Warren and Finkener. The specimens examined showed the different densities 6·39, 6·078, 5·976, 5·971, 5·860, 5·701, and 5·698. Those with the highest densities had a black powder, while in the lighter specimens the powder was more of a chocolate-brown or cherry-red color. The oxygen ratios between the bases and the hypocolumbic acid in the analyses were: 1 : 4·07, 1 : 3·95, 1 : 3·7, 1 : 3·87, 1 : 3·56, 1 : 3·53, 1 : 3·4, 1 : 3·34, and 1 : 3·16.

The specimens of columbite from Connecticut showed less alteration than those from Bodenmais. Among them, however, were some specimens with a high specific gravity and having a black powder. Four analyses by Rose, Schlieper, Chandler and Oesten, on specimens having the densities 6·048, 5·583, 5·708, 5·483, showed the same peculiarities in regard to streak and powder as observed in the Bodenmais mineral. The oxygen ratios were 1 : 3·63, 1 : 3·48, 1 : 3·13 and 1 : 3·1. The specimens which contained the largest amount of hypocolumbic acid had the highest density, and had also a black powder.

Rose observes that the columbite occurring in the Greenland cryolite is unquestionably the purest yet found. The crystals have suffered no decomposition, and all the specimens have therefore the same specific gravity. Selected fragments gave this density as 5·374 to 5·376, in powder 5·4; the powder had a light cherry-red color. Two analyses by Oesten, and one by Finkener gave the oxygen ratio of bases to acid as 1 : 3·08, 1 : 3·14 and 1 : 3·11. The columbite from the Ilmen Mts. is also extremely pure. Different specimens have the same specific gravity, G.=5·461—5·447. An analysis of this columbite by Oesten shows it to contain a small amount of oxyd of uranium; this is not found in columbite from other localities. The Ural columbite does not contain any yttria or magnesia, as was formerly supposed. The relation of the oxygen of the bases to the acid is as 1 : 3·06.

From these analyses, Rose concludes that in the purer varieties of columbite—those which have suffered no alteration or decomposition—the oxygen of the hypocolumbic acid is three times that of the oxygen of the protoxyd of iron and manganese, that is, the relation of oxygen of the acid to that of the bases is as 3 : 1. An analogous relation exists in wolfram, the ratio between the tungstic acid and the bases being 3 : 1, and the bases also consist of protoxyd of iron and manganese.—*Jour. prakt. Chem.*, lxxxv, 438. G. J. B.

2. *Kischtimite, a new mineral.*—T. Korovaeff describes, under the name *Kischtim-Parisit*, a new mineral from the gold-washings of the Borsowka river in the district of Kischtim in the Ural Mountains (*Jour. prakt. Chem.*, lxxxv, 442). The mineral was not crystallized; color dark brownish-yellow, streak much lighter; fracture sub-conchoidal; lustre between greasy and vitreous; friable, and in small pieces transparent. G.=4·784. B.B., at a moderate temperature loses its lustre, becomes of a dull, opaque, opaline yellow color; at a high temperature it glows, and on cooling has a high lustre, and a brick-red color. In the closed tube

gives off water and becomes darker colored. Soluble in the fluxes: with borax in the outer flame gives a yellow glass, in the inner flame faint yellow, which on cooling becomes colorless; with salt of phosphorus gives the same reactions, except that both beads are colorless on cooling. The powder moistened with sulphuric acid gives off fluohydric acid. Dissolved in chlorhydric acid, with the evolution of carbonic acid, and traces of chlorine. The acid solution gave no precipitate with sulphydric acid gas; in the neutral solution sulphid of ammonium gave a colorless voluminous precipitate, insoluble in caustic potash, but soluble in excess of carbonate of ammonia, thus indicating the presence of the oxyds of the cerium metals. The filtrate from the sulphid of ammonium precipitate left no residue on evaporation and ignition, showing the absence of alkalies and alkaline earths. Three analyses gave:

	Ć	Ḣ	La	Ce	Fl	O (loss)
1.	17·19	2·20	37·46	26·78	6·12	
2.	19·65		35·66	28·84	5·97	
3.	19·30		——	——	6·96	
Mean,	17·19	2·20	36·56	27·81	6·35	9·89

From this, Korovaeff deduces the formula $6\dot{L}aĆ+(\dot{C}e+Ce_2Fl_3+2\dot{H})$ or $3\dot{L}aĆ+Ce_2(Fl,O)^3+\dot{H}$, which on calculation equals Ć 17·28, Ḣ 2·40, La 37·67, Ce 25·23, Fl 7·52, O 9·60=100·00.

In physical characters, except crystallization, the mineral very closely approaches the *parisite* from Musso, described and analyzed by Bunsen. The Kischtim mineral, however, contains no lime, and on treatment with chlorhydric acid evolves chlorine, indicating the presence of sesquioxyd of cerium. Its specific gravity also differs materially from parisite, the latter being 4·35, while the Kischtim mineral is 4·784. We suggest the name *Kischtimite* as preferable to the double name *Kischtim-Parisit* for this new species. G. J. B.

3. *Catalogue of the Miocene Shells of the Atlantic Slope;* by T. A. Conrad.—In the last number of the *Proceedings of the Academy of Natural Sciences* of Philadelphia (Dec., 1862, pp. 559–582), Mr. Conrad has given us a Catalogue which must prove an invaluable assistance, not only to palæontologists but to those students of recent zoology who make the shells of our coast a subject of study. The notices of our Miocene shells previously published, particularly those of Mr. Conrad himself, are so numerous and scattered, that to collect and index them, —a necessary preliminary to their investigation,—would be by far too extended a work for every one interested therein to undertake for himself. It is a subject for congratulation that this work is now done for all, by one so competent and so well acquainted with our Tertiary Faunæ as the distinguished author of the "Catalogue."

Mr. Conrad states that the Miocene of our Atlantic slope extends from New Jersey to South Carolina, and he includes in it the so-called "Pliocene" of the latter State. The newer Pliocene or Pleistocene of the coast rests immediately upon the Miocene, and there is no intermingling of extinct forms between these two formations. About 580 species of shells are found in the Miocene, the proportion of Conchifers to Gasteropods being 1 : 1·14. Mr. C. thinks that the proportion of recent species generally acknowledged to occur in this formation should

be greatly reduced, and he rejects from the list no less than 18 which were formerly supposed to be identical with recent forms. He even doubts whether any of them are identical; those that possibly are so being only 30 in number, most of which he has not as yet had an opportunity of comparing with their recent analogues or co-species. The *Natica heros* and *N. duplicata* of our coast have fossil analogues so closely resembling them that he can find no essential difference between them. We would suggest that, in making these comparisons, specimens from the intermediate Pleistocene deposits should also be examined. If a few of the denizens of that ancient Miocene sea have really survived to the present epoch, we might expect them to have undergone some slight change during so great a lapse of time. A comparison with those examples which lived at an intermediate epoch would go far toward settling this question.

A few words on the nomenclature employed by Mr. Conrad will not be out of place here. He has followed the authors of "The Genera of Recent Mollusca" in restoring the "generic" names of Klein, and giving them the preference over the more recent, but well established names of authors whose works were, until very lately, universally accepted. Against this we must enter our protest. The generic nomenclature of the Mollusca is in a state of extreme confusion, and unless we can find some principle to guide us in selection among the older names of these groups of species, we can never reach a firm basis. The true principle seems to be sufficiently plain, and has, with few exceptions, been generally followed.

The system of nomenclature proposed by Linnæus, the convenience of which has been proved by an hundred years of usage, has become the *law*, and it follows that no names should be adopted as generic which were not given in accordance with this law, and by those who understood that they were naming *genera*, in the Linnæan sense. We would not, indeed, join those who consider that we cannot go behind a particular edition of the "*Systema Naturæ*," nor would we exclude from use the names proposed by all authors who were not strictly binomial in their specific nomenclature. An author may name a genus, and give its type, without describing any species whatever under it. But it is essential that an author should acknowledge genera as understood by Linnæus, and also that they are composed of species in the Linnæan sense. But Klein, who was an avowed enemy of the Linnæan school, made use of an entirely different system in his "*Tentamen Methodi Ostracologicæ*." His "species" avowedly include several species of the Linnæan kind, each designated by a "phrase" according to the general usage among pre-Linnæan authors. Witness, for instance, on p. 114 of his work above named, where we find,—"Genus I, *Patella integra*. Species I, *Striata*. 1. *Indica, major, striis planis*," etc. 2. "*Cypria, striis crassis*," etc., and so on, including 22 species under the "species *Striata*," until we come to "Species II, *Reticulata* seu *clathrata*" with 7 species; "Species III, *Virgata* vel *circinnata*," with 4 species; "Species IV, *Lævis*," with 5, and so on. Klein's *species* in fact correspond more nearly with Linnæan *genera*. Again, very many of his generic names are composed of two words, and the most enthusiastic of his modern followers do not

claim priority for these, with the exception of H. and A. Adams, who take sometimes the first word of the binomial phrase for the generic name to be established! To this objection it is answered that wherever Klein's names are (accidentally) uninomial, they must be adopted. But if this method of settling the question be allowed, we shall have all writers who are affected with the prevailing rage for change, searching through every forgotten and almost extinct work at all relating to natural history, which has appeared since the invention of printing, seizing upon every case in which an author *happened* to designate a group of animals by a single name, and adding this to our already overburdened synonymy, to the suppression of the name last in vogue. And further, when we have got to the date of the invention of printing, there will be some who will insist that the distribution of a certain number of copies of a manuscript constitutes publication, and so go back to the still older names given by writers who flourished during and before the Middle Ages! Naturalists, averse to the more severe studies in the realm of nature herself, will become archæologists, and cheaply earn reputation by seeking for prior names to replace those which have become well known.

We have thus dwelt upon Klein's case because it is one of importance, not only as a type of many, but on account of the great number (about 170) of "generic" names which he proposed, a large number of which the authors of the Moerch and Adams school are now endeavoring to revive, and nearly all of which are liable to be pressed upon us with as much reason as the first lot has been.

But to drop the question of simple nomenclature, we may, in conclusion, express the lively satisfaction with which we have examined the generic distribution of our Miocene shells by the author of the "*Catalogue.*" Numbers of species formerly huddled together in old generic groups are here separated and given their proper distinct place in the system, and the names proposed for the new genera are for the most part very appropriate. We are now enabled satisfactorily to compare our Miocene fauna with that of Europe, and with that of the actual epoch on our own coast. W. S.

4. *Geology of Vermont.*—Announcement has been issued by A. D. Hager of Proctorsville, Vt., that the Geological Report of Vermont in quarto will hereafter be disposed of by him at six dollars a copy. The edition is nearly exhausted.

VI. BOTANY AND ZOOLOGY.

1. *A new character in the Fruit of Oaks.—Note sur un Nouveau Caractère observè dans le Fruit des Chênes et sur la meilleure division à adopter pour le Genre Quercus*, is the title of a short article contributed by Mr. Alph. DeCandolle to the *Bibliothèque Universelle* for October, 1862. It is well known to botanists that Mr. DeCandolle has been assiduously engaged in the elaboration of the order *Cupuliferæ* for the Prodromus, and has had before him the authentic types of almost every published species, and an amount of materials as to many of them which, so far as dried specimens may serve, leaves little to be asked. The present paper has a purely botanical interest, and, having been already reproduced in the *Edinburgh Journal of Science*, need only be briefly

noticed here. The new character unexpectedly brought to light is that of the position in the acorn of the five atrophied ovules as respects the seed, or kernel, which results from the fertilization of the sixth ovule, the only one which ever matures. DeCandolle shows that the aborted ovules do not disappear as the fruit grows, but persist, just as they are well known to do in the Horse-chestnut and Buckeye, and that they may be found in the ripe acorn upon examination. It appears that the ovules in this genus are by no means always suspended from the summit of the cells, as generally thought, indeed, that they are ascending from the base or near it in the common European Oak, and persist there in the acorn; while, however, they are found above the seed in four of the five natural sections under which DeCandolle arranges the species. But in the great section *Lepidobalanus*, which comprises all the Oaks of the northern parts of the world, except one of California, these ovules are situated sometimes at the base, rarely about the middle, and sometimes near the summit of the seed. Moreover, all the Oaks which mature their fruit the first year bear their atrophied ovules at the base of the seed, or at least below its middle. Oaks of biennial maturation are divided in this respect, some having these ovules below, others above; but most of the North American species appear to be in the latter category.

This character of annual or biennial maturation, which DeCandolle thinks has been neglected for half a century [surely not in this country, where it has been familiar, both popularly and scientifically, ever since the time of the elder Michaux], was taken up by the acute and excellent Gay in the Old World, who showed that two Cork Oaks had been confounded under the name of *Quercus Suber*, one with annual, the other with biennial maturation of the fruit. This character, being of easy application to herbarium specimens of any goodness, as well as obvious in the living tree, would naturally be much relied on in classification. But, as in the case of the two Cork Oaks, so in general, it is not coördinated with other important differences, and therefore it serves merely to distinguish related species, or to subdivide conveniently that portion of the *Lepidobalanus* group in which the ovules are inferior.

DeCandolle notices a peculiarity in the embryo of our Live Oak (*Q. virens*), viz.: that its cotyledons are perfectly united into one homogeneous substance, while nothing of the sort appears in its near relative *Q. Ilex* of the Old World. He wishes the germination of the Live Oak to be examined in this respect, apparently to determine whether the mass consists of two united cotyledons or of a single cylindrical one. *A priori*, we could have little doubt; but we solicit fresh acorns of the coming season, or germinating ones the present year from some southern posts. A. G.

2. *Species, considered as to Variation, Geographical Distribution, and Succession.—Etude sur l'Espèce, à l'occasion d'une Revision de la Famille des Cupulifers, par* M. Alph. DeCandolle.—This is the title of a second paper by Mr. DeCandolle growing out of his study of the Oaks. It was published in the November number of the *Bibliothèque Universelle*, and separately issued as a pamphlet. A less inspiring task could hardly be assigned to a botanist than the systematic elaboration of the genus *Quercus* and its allies. The vast materials assembled under DeCandolle's hands, while disheartening for their bulk, offered small hope of novelty. The subject was both extremely trite and extremely difficult.

Happily it occurred to DeCandolle that an interest might be imparted to an onerous undertaking, and a work of necessity be turned to good account for science, by studying the Oaks in view of the question of *Species*.

What this term *Species* means, or should mean, in natural history, what the limits of species, *inter se* or chronologically, or in geographical distribution, their modifications, actual or probable, their origin, and their destiny,—these are questions which surge up from time to time; and now and then in the progress of science they come to assume a new and hopeful interest. Botany and Zoology, Geology, and what our author, feeling the want of a new term, proposes to name *Epiontology*,[1] all lead up to and converge into this class of questions, while recent theories shape and point the discussion. So we look with eager interest to see what light the study of Oaks, by a very careful, experienced, and conservative botanist, particularly conservant with the geographical relations of plants, may throw upon the subject.

The course of investigation in this instance does not differ from that ordinarily pursued by working botanists; nor, indeed, are the theoretical conclusions other than those to which a similar study of other orders might not have equally led. The Oaks afford a very good occasion for the discussion of questions which press upon our attention, and perhaps they offer peculiarly good materials on account of the number of fossil species.

Preconceived notions about species being laid aside, the specimens in hand were distributed, according to their obvious resemblances, into groups of apparently identical or nearly identical forms, which were severally examined and compared. Where specimens were few, as from countries little explored, the work was easy, but the conclusions, as will be seen, of small value. The fewer the materials, the smaller the likelihood of forms intermediate between any two, and—what does not appear being treated upon the old law-maxim as non-existent—species are readily enough defined. Where, however, specimens abound, as in the case of the Oaks of Europe, of the Orient, and of the United States, of which the specimens amounted to hundreds, collected at different ages, in varied localities, by botanists of all sorts of views and predilections,—here alone were data fit to draw useful conclusions from. Here, as DeCandolle remarks, he had every advantage, being furnished with materials more complete than any one person could have procured from his own herborizations, more varied than if he had observed a hundred times over the same forms in the same district, and more impartial than if they had all been amassed by one person with his own ideas or predispositions. So that vast herbaria, into which contributions from every source have flowed for years, furnish the best possible data,—at least are far better than any practicable amount of personal herborization,—for the comparative study of related forms occurring over wide tracts of territory. But as the mate-

[1] A name which, at the close of his article, DeCandolle proposes for *the study of the succession of organized beings*, to comprehend, therefore, palæontology and all included under what is called geographical botany and zoology,—the whole forming a science parallel to geology,—the latter devoted to the history of unorganized bodies, the former, to that of organized beings, as respects origin, distribution, and succession. We are not satisfied with the word, notwithstanding the precedent of *palæontology*; since *ontology*, the science of being, has an established meaning as referring to mental existence,—i. e., is a synonym or a department of metaphysics.

rials increase, so do the difficulties. Forms, which appeared totally distinct, approach or blend through indermediate gradations; characters, stable in a limited number of instances or in a limited district, prove unstable occasionally, or when observed over a wider area; and the practical question is forced upon the investigator,—what here is probably fixed and specific, and what is variant, pertaining to individual, variety or race?

In the examination of these rich materials, certain characters were found to vary upon the same branch, or upon the same tree, sometimes according to age or development, sometimes irrespective of such relations or of any assignable reasons. Such characters, of course, are not specific, although many of them are such as would have been expected to be constant in the same species, and are such as generally enter into specific definitions. Variations of this sort, DeCandolle, with his usual painstaking, classifies and tabulates, and even expresses numerically their frequency in certain species. The results are brought well to view in a systematic enumeration,—

(1.) Of characters which *frequently* vary upon the same branch: over a dozen such are mentioned.

(2.) Of those which *sometimes* vary upon the same branch: a smaller number of these are mentioned.

(3.) Those so rare that they might be called monstrosities.

Then he enumerates characters, ten in number, which he has never found to vary on the same branch, and which, therefore, may better claim to be employed as specific. But, as among them he includes the duration of the leaves, the size of the cupule, and the form and size of its scales, which are by no means wholly uniform in different trees of the same species, even these characters must be taken with allowance. In fact, having first brought together, as groups of the lowest order, those forms which varied upon the same stock, he next had to combine similarly various forms which, though not found associated upon the same branch, were thoroughly blended by intermediate degrees.

"The lower groups (varieties or races) being thus constituted, I have given the rank of *species* to the groups next above these, which differ in other respects, i. e., either in characters which were not found united upon certain individuals, or in those which do not show transitions from one individual to another. For the Oaks of regions sufficiently known, the species thus formed rest upon satisfactory bases, of which the proof can be furnished. It is quite otherwise with those which are represented in our herbaria by single or few specimens. These are *provisional species*,—species which may hereafter fall to the rank of simple varieties. I have not been inclined to prejudge such questions; indeed, in this regard, I am not disposed to follow those authors whose tendency is, as they say, to reunite species. I never reunite them without proof in each particular case; while the botanists to whom I refer do so on the ground of analogous variations or transitions occurring in the same genus or in the same family. For example, resting on the fact that *Quercus Iler*, *Q. coccifera*, *Q. acutifolia*, &c., have the leaves sometimes entire and sometimes toothed upon the same branch, or present transitions from one tree to another, I might readily have united my *Q. Tlapuxahuensis* to *Q. Sartorii* of Liebmann, since these two differ only in their entire or their toothed leaves. From the fact that the length of the peduncle varies in *Q. Robur* and many other Oaks, I might have combined *Q. Seemannii* Liebm., with *Q. salicifolia* Née. I have not admitted these inductions, but have demanded visible proof in each particular case. Many

species are thus left as provisional; but in proceeding thus, the progress of the science will be more regular, and the synonymy less dependent upon the caprice or the theoretical opinions of each author."

This is safe and to a certain degree judicious, no doubt, as respects published species. Once admitted, they may stand until they are put down by evidence, direct or circumstantial. Surely a species may rightfully be condemned on good circumstantial evidence. But what course does DeCandolle pursue in the case—of every-day occurrence to most working botanists having to elaborate collections from countries not so well explored as Europe—when the forms in question, or one of the two, are as yet unnamed? Does he introduce as a new species every form which he cannot connect by ocular proof with a near relative, from which it differs only in particulars which he sees are inconstant in better known species of the same group? We suppose not. But if so, little improvement for the future upon the state of things revealed in the following parapraph can be expected.

"In the actual state of our knowledge, after having seen nearly all the original specimens, and in some species as many as 200 representatives from different localities, I estimate that, out of the 300 species of *Cupuliferæ* which will be enumerated in the Prodromus, two-thirds at least are *provisional* species. In general, when we consider what a multitude of species were described from a single specimen, or from the forms of a single locality, of a single country, or are badly described, it is difficult to believe that above one-third of the actual species in botanical works will remain unchanged."

Such being the results of the *want* of adequate knowledge, how is it likely to be when our knowledge is largely increased? The judgment of so practiced a botanist as DeCandolle is important in this regard, and it accords with that of other botanists of equal experience.

"They are mistaken," he pointedly asserts, "who repeat that the greater part of our species are clearly limited, and that the doubtful species are in a feeble minority. This seemed to be true, so long as a genus was imperfectly known, and its species were founded upon few specimens, that is to say, were provisional. Just as we come to know them better, intermediate forms flow in, and doubts as to specific limits augment."

DeCandolle insists, indeed, in this connection, that the higher the rank of the groups, the more definite their limitation, or, in other terms, the fewer the ambiguous or doubtful forms; that genera are more strictly limited than species, tribes than genera, orders than tribes, &c. We are not convinced of this. Often where it has appeared to be so, advancing discovery has brought intermediate forms to light, perplexing to the systematist. "They are mistaken," we think more than one systematic botanist will say, "who repeat that the greater part of our natural orders and tribes are absolutely limited," however we may agree that we will limit them. Provisional genera we suppose are proportionally hardly less common than provisional species; and hundreds of genera are kept up on considerations of general propriety or general convenience, although well known to shade off into adjacent ones by complete gradations. Somewhat of this greater fixity of higher groups, therefore, is rather apparent than real. On the other hand, that varieties should be less definite than species, follows from the very terms employed. They are ranked as varieties, rather than species, just because of their less definiteness.

Singular as it may appear, we have heard it denied that spontaneous varieties occur. DeCandolle makes the important announcement that, in the Oak genus, the best known species are just those which present the greatest number of spontaneous varieties and sub-varieties. The maximum is found in *Q. Robur*, with twenty-eight varieties, all spontaneous. Of *Q. Lusitanica* eleven varieties are enumerated, of *Q. Calliprinos* ten, of *Q. coccifera* eight, &c. And he significantly adds that "these very species which offer such numerous modifications are themselves ordinarily surrounded by other forms, provisionally called species, because of the absence of known transitions or variations, but to which some of these will probably have to be joined hereafter." The inference is natural, if not inevitable, that the difference between such species and such varieties is only one of degree, either as to amount of divergence, or of hereditary fixity, or as to the frequency or rarity, at the present time, of intermediate forms.

This brings us to the second section of DeCandolle's article, in which he passes on, from the observation of the present forms and affinities of Cupuliferous plants, to the consideration of their probable history and origin. Suffice it to say, that he frankly accepts the inferences derived from the whole course of observation, and even contemplates with satisfaction a probable historical connexion between congeneric species. He accepts and, by various considerations drawn from the geographical distribution of European *Cupuliferæ*, fortifies the conclusion—long ago arrived at by Edward Forbes—that the present species, and even some of their varieties, date back to about the close of the Tertiary epoch, since which time they have been subject to frequent and great changes of habitation or limitation, but without appreciable change of specific form or character; that is, without profounder changes than those within which a species at the present time is known to vary. Moreover, he is careful to state that he is far from concluding that the time of the appearance of a species in Europe at all indicates the time of its origin. Looking back still further into the Tertiary epoch, of which the vegetable remains indicate many analogous, but few, if any, identical forms, he concludes, with Heer and others, that specific changes of form, as well as changes of station, are to be presumed. And finally, that "the theory of a succession of forms through the deviation of anterior forms is the most natural hypothesis, and the most accordant with the known facts in palæontology, geographical botany and zoology, of anatomical structure and classification: but direct proof of it is wanting, and moreover, if true, it must have taken place very slowly; so slowly indeed, that its effects are discernable only after a lapse of time far longer than our historic epoch."

In contemplating the present state of the species of *Cupuliferæ* in Europe, DeCandolle comes to the conclusion that, while the Beech is increasing, and extending its limits southward and westward (at the expense of *Coniferæ* and Birches), the Common Oak, to some extent, and the Turkey oak decidedly, are diminishing and retreating, and this wholly irrespective of man's agency. This is inferred of the Turkey Oak from the great gaps found in its present geographical area, which are otherwise inexplicable, and which he regards as plain indications of a partial extinction. Community of descent of all the individuals of species is of course implied in these and all similar reasonings.

An obvious result of such partial extinction is clearly enough brought to view. The European Oaks (like the American species) greatly tend to vary,—that is, they manifest an active disposition to produce new forms. Every form tends to become hereditary, and so to pass from the state of mere variation to that of race; and of these competing incipient races some only will survive. *Quercus Robur* offers a familiar illustration of the manner in which one form may in the course of time become separated into two or more distinct ones.

To Linnæus this Common Oak of Europe was all of one species. But of late years the greater number of European botanists have regarded it as including three species, *Q. pedunculata*, *Q. sessiliflora*, and *Q. pubescens*. DeCandolle looks with satisfaction to the independent conclusion which he reached from a long and patient study of the forms (and which Webb, Gay, Bentham and others had equally reached), that the view of Linnæus was correct, inasmuch as it goes to show that the idea and the practical application of the term *species* have remained unchanged during the century which has elapsed since the publication of the *Species Plantarum*. But the idea remaining unchanged, the facts might appear under a different aspect, and the conclusion be different, under a slight and very supposable change of circumstances. Of the twenty-eight spontaneous varieties of *Q. Robur*, which DeCandolle recognizes, all but six, he remarks, fall naturally under the three sub-species, *pedunculata*, *sessiliflora*, and *pubescens*, and are therefore forms grouped around these as centres; and, moreover, the few connecting forms are by no means the most common. Were these to die out, it is clear that the three forms which have already been so frequently taken for species, would be what the group of four or five provisionally admitted species which closely surround *Q. Robur* (see p. 435) now are. The best example of such a case, as having in all probability occurred, through geographical segregation and partial extinction, is that of the Cedar, thus separated into the Deodar, the Lebanon, and the Atlantic Cedars,—a case admirably worked out by Dr. Hooker two or three years ago.[2]

A special advantage of the *Cupuliferæ* for determining the probable antiquity of existing species in Europe, DeCandolle finds in the size and character of their fruits. However it may be with other plants (and he comes to the conclusion generally that marine currents and all other means of distant transport have played only a very small part in the actual dispersion of species), the transport of acorns and chestnuts by natural causes across an arm of the sea in a condition to germinate, and much more the spontaneous establishment of a forest of Oaks or Chestnuts in this way, DeCandolle conceives to be fairly impossible in itself, and contrary to all experience. From such considerations, i. e., from the actual dispersion of the existing species, with occasional aid from Post-tertiary deposits, it is thought to be shown that the principal *Cupuliferæ* of the Old World attained their actual extension before the present separation of Sicily, Sardinia and Corsica, or of Britain, from the European continent.

This view once adopted, and this course once entered upon, has to be pursued farther. *Quercus Robur* of Europe with its bevy of admitted

[2] Nat. Hist. Review, Jan., 1862. See this Journal, [2], xxxiv, 148.

derivatives, and its attending species only provisionally admitted to that rank, is very closely related to certain species of Eastern Asia, and of Oregon and California,—so closely that "a view of the specimens by no means forbids the idea that they have all originated from *Q. Robur*, or have originated, with the latter, from one or more preceding forms so like the present ones that a naturalist could hardly know whether to call them species or varieties." Moreover, there are fossil leaves from diluvian deposits in Italy, figured by Gaudin, which are hardly distinguishable from those of *Q. Robur* on the one hand, and from those of *Q. Douglasii*, &c., of California, on the other. No such leaves are found in any Tertiary deposit in Europe; but such are found of that age, it appears, in Northwest America, where their remote descendants still flourish. So that the probable genealogy of *Q. Robur*, traceable in Europe up to the commencement of the present epoch, looks eastward and far into the past on far distant shores.

Q. Ilex, the Evergreen Oak of Southern Europe and Northern Africa, reveals a similar archæology; but its presence in Algeria leads DeCandolle to regard it as a much more ancient denizen of Europe than *Q. Robur;* and a Tertiary Oak, *Q. ilicoides*, from a very old Miocene bed in Switzerland, is thought to be one of its ancestral forms. This high antiquity once established, it follows almost of course that the very nearly related species in Central Asia, in Japan, in California, and even our own Live Oak with its Mexican relatives, may probably enough be regarded as early offshoots from the same stock with *Q. Ilex.*

In brief,—not to continue these abstracts and remarks, and without reference to Darwin's particular theory (which DeCandolle at the close very fairly considers),—if existing species, or many of them, are as ancient as they are now generally thought to be, and were subject to the physical and geographical changes (among them the coming and the going of the Glacial epoch) which this antiquity implies; if in former times they were as liable to variation as they now are; and if the individuals of the same species may claim a common local origin, then we cannot wonder that "the theory of a succession of forms by deviations of anterior forms" should be regarded as "the most natural hypothesis," nor at the general advance made towards its acceptance in some form or other.

The question being, not, how plants and animals originated, but, how came the existing animals and plants to be just where they are and what they are? it is plain that naturalists interested in such inquiries are mostly looking for the answer in one direction. The general drift of opinion, or at least of expectation, is exemplified by this essay of DeCandolle; and the set and force of the current are seen by noticing how it carries along naturalists of widely different views and prepossessions—some faster and farther than others,—but all in one way. The tendency is, we may say, to extend the law of continuity, or something analogous to it, from inorganic to organic nature, and in the latter to connect the present with the past in some sort of material connection. The generalization may indeed be expressed so as not to assert that the connection is genetic, as in Mr. Wallace's formula: "Every species has come into

existence coincident both in time and space with preëxisting closely allied species." Edward Forbes, who may be called the originator of this whole line of inquiry, long ago expressed a similar view. But the only material sequence we know, or can clearly conceive, in plants and animals, is that from parent to progeny; and, as DeCandolle implies, the origin of species and that of races can hardly be much unlike, nor governed by other than the same laws, whatever these may be.

The progress of opinion upon this subject in one generation is not badly represented by that of DeCandolle himself, who is by no means prone to adopt new views without much consideration. In an elementary treatise published in the year 1835, he adopted and, if we rightly remember, vigorously maintained, Schouw's idea of the double or multiple origin of species, at least of some species,—a view which has been carried out to its ultimate development only perhaps by Agassiz, in the denial of any necessary genetic connection among the individuals of the same species, or of any original localization more restricted than the area now occupied by the species. But in 1855, in his *Géographie Botanique*, the multiple hypothesis, although in principle not abandoned, is seen to lose its point, in view of the probable high antiquity of existing species. The actual vegetation of the world being now regarded as a continuation, through numerous geological, geographical, and more recently historical, changes, of anterior vegetations, the actual distribution of plants is seen to be a consequence of preceding conditions, and geological considerations, and these alone may be expected to explain all the facts, many of them so curious and extraordinary, of the actual geographical distribution of the species. In the present essay, not only the distribution but the origin of congeneric species is regarded as something derivative; whether derived by slow and very gradual changes in the course of ages, according to Darwin, or by a sudden, inexplicable change of their Tertiary ancestors, as conceived by Heer, DeCandolle hazards no opinion. It may, however, be inferred that he looks upon 'natural selection' (which he rather underrates) as a real, but insufficient, cause; while some curious remarks (pp. 57–58), upon the number of monstrosities annually produced, and the possibility of their enduring, may be regarded as favorable to Heer's view.

As an index to the progress of opinion in the direction referred to, it will be interesting to compare Sir Charles Lyell's well known chapters of 20 or 30 years ago, in which the permanence of species was ably maintained, with his treatment of the same subject in a work just issued in England, which, however, has not yet reached us.

A belief in the derivation of species may be maintained along with a conviction of great persistence of specific characters. This is the idea of the excellent Swiss vegetable palæontologist, Heer, who imagines a sudden change of specific type at certain periods, and perhaps is that of Pictet. Falconer adheres to somewhat similar views in his elaborate paper on Elephants, living and fossil, in the *Natural History Review* for January last. Noting that "there is clear evidence of the true Mammoth having existed in America long after the period of the northern drift, when the surface of the country had settled down into its present form," and also in Europe so late as to have been a cotemporary of the Irish Elk,

and on the other hand that it existed in England so far back as before the deposition of the boulder Clay; also that four well-defined species of fossil Elephant are known to have existed in Europe; that "a vast number of the remains of three of these species have been exhumed over a large area in Europe; and, even in the geological sense, an enormous interval of time has elapsed between the formation of the most ancient and the most recent of these deposits, quite sufficient to test the persistence of specific characters in an Elephant," he presents the question: "Do then the successive Elephants occurring in these strata show any signs of a passage from the older form into the newer?"

To which the reply is: "If there is one fact which is impressed on the conviction of the observer with more force than any other, it is the persistence and uniformity of the characters of the molar teeth in the earliest known Mammoth and his most modern successor .... Assuming the observation to be correct, what strong proof does it not afford of the persistence and constancy, throughout vast intervals of time, of the distinctive characters of those organs which are most concerned in the existence and habits of the species? If we cast a glance back on the long vista of physical changes which our planet has undergone since the Neozoic Epoch, we can nowhere detect signs of a revolution more sudden and pronounced, or more important in its results, than the intercalation and sudden disappearance of the glacial period. Yet the 'dicyclotherian' Mammoth lived before it, and passed through the ordeal of all the hard extremities it involved, bearing his organs of locomotion and digestion all but unchanged. Taking the group of four European fossil species above enumerated, do they show any signs in the successive deposits of a transition from the one form into the other? Here again the result of my observation, in so far as it has extended over the European area, is, that the specific characters of the molars are constant in each, within a moderate range of variation, and that we nowhere meet with intermediate forms." .... Dr. Falconer continues, (p. 80):

"The inferences which I draw from these facts are not opposed to one of the leading propositions of Darwin's theory. With him, I have no faith in the opinion that the Mammoth and other extinct Elephants made their appearance suddenly, after the type in which their fossil remains are presented to us. The most rational view seems to be, that they are in some shape the modified descendants of earlier progenitors. But if the asserted facts be correct, they seem clearly to indicate that the older elephants of Europe, such as *E. meridionalis* and *E. antiquus*, were not the stocks from which the later species, *E. primigenius* and *E. Africanus* sprung, and that we must look elsewhere for their origin. The nearest affinity, and that a very close one, of the European *E. meridionalis* is with the Miocene *E. planifrons* of India; and of *E. primigenius*, with the existing India species.

"Another reflexion is equally strong in my mind,—that the means which have been adduced to explain the origin of the species by 'Natural Selection,' or a process of variation from external influences, are inadequate to account for the phenomena. The law of phyllotaxis, which governs the evolution of leaves around the axis of a plant, is as nearly constant in its manifestation as any of the physical laws connected with the material world. Each instance, however different from another, can be shown to be a term of some series of continued fractions. When this is coupled with the geometrical law governing the evolution of form, so manifest in some departments of the animal kingdom, e. g., the spiral shells of the Mollusca, it is difficult to believe that there is not, in

nature, a deeper-seated and innnate principle, to the operation of which Natural Selection is merely an adjunct. The whole range of the Mammalia, fossil and recent, cannot furnish a species which has had a wider geographical distribution, and passed through a longer term of time, and through more extreme changes of climatal conditions, than the Mammoth. If species are so unstable, and so susceptible of mutation through such influences, why does that extinct form stand out so signally a monument of stability? By his admirable researches and earnest writings, Darwin has, beyond all his cotemporaries, given an impulse to the philosophical investigation of the most backward and obscure branch of the biological sciences of his day; he has laid the foundations of a great edifice; but he need not be surprised, if, in the progress of erection, the superstructure is altered by his successors, like the Duomo of Milan from the Roman to a different style of architecture."

Entertaining ourselves the opinion that something more than natural selection is requisite to account for the orderly production and succession of species, we offer two incidental remarks upon the above extract.

First, we find in it,—in the phrase "Natural Selection, or a process of variation from external influences,"—an example of the very common confusion of two distinct things, viz., *variation* and *natural selection*. The former has never yet been shown to have its cause in "external influences," nor to occur at random. As we have elsewhere insisted, if not inexplicable, it has never been explained; all we can yet say is, that plants and animals are prone to vary, and that some conditions favor variation. Perhaps in this Dr. Falconer may yet find what he seeks: for "it is difficult to believe that there is not in [its] nature, a deeper-seated and innate principle, to the operation of which Natural Selection is merely an adjunct." The latter, which is the *ensemble* of the external influences, including the competition of the individuals themselves, picks out certain variations as they arise, but in no proper sense can be said to originate them.

Secondly, although we are not quite sure how Dr. Falconer intends to apply the law of phyllotaxis to illustrate his idea, we fancy that a pertinent illustration may be drawn from it, in this way. There are two *species* of phyllotaxis, perfectly distinct, and, we suppose, not mathematically reducible the one to the other,—viz. 1, that of alternate leaves, with its varieties; and, 2, that of verticillate leaves, of which opposite leaves present the simplest case. That, although generally constant, a change from one variety of alternate phyllotaxis to another should occur on the same axis, or on successive axes, is not surprising, the different sorts being terms of a regular series,—although indeed we have not the least idea as to how the change from the one to the other comes to pass. But it is interesting, and in this connection perhaps instructive, to remark that, while some dicotyledonous plants hold to the verticillate, i. e., opposite-leaved phyllotaxis throughout, a larger number—through the operation of some deep-seated and innate principle, which we cannot fathom,—change abruptly into the other species at the second or third node, and change back again in the flower, or else effect a synthesis of the two species in a manner which is puzzling to understand. Here is a change from one fixed law to another, as unaccountable, if not as great, as from one specific form to another.

An elaborate paper on the vegetation of the Tertiary period in the southeast of France, by Count Gaston de Saporta, published in the *Ann.*

Sci. Nat. in 1862, vol. xvi, pp. 309–344,—which we have not space to analyze,—is worthy of attention from the general inquirer, on account of its analysis of the Tertiary flora into its separate types, Cretaceous, Austral, Tropical, and Boreal, each of which has its separate and different history, —and for the announcement that "the *hiatus*, which, in the idea of most geologists, intervened between the close of the Cretaceous and the beginning of the Tertiary, appears to have had no existence, so far as concerns the vegetation; that in general it was not by means of a total overthrow, followed by a complete new emission of species, that the flora has been renewed at each successive period; and that while the plants of Southern Europe inherited from the Cretaceous period more or less rapidly disappeared, as also the austral forms, and later the tropical types (except the Laurel, the Myrtle, and the Chamærops humilis), the boreal types, coming later, survived all the others, and now compose, either in Europe, or in the north of Asia, or in North America, the basis of the actual arborescent vegetation. Especially "a very considerable number of forms nearly identical with Tertiary forms now exist in America, where they have found, more easily than in our [European] soil—less vast and less extended southward—refuge from ulterior revolutions." The extinction of species is attributed to two kinds of causes; the one material or physical, whether slow or rapid; the other inherent in the nature of organic beings, incessant, but slow, in a manner latent, but somehow assigning to the species, as to the individuals, a limited period of existence, and, in some equally mysterious but wholly natural way, connected with the development of organic types:—"By *type* meaning a collection of vegetable forms constructed upon the same plan of organization, of which they reproduce the essential lineaments with certain secondary modifications, and which appear to run back to a common point of departure."

In this community of types, no less than in the community of certain existing species, Saporta recognizes a prolonged material union between North America and Europe in former times. Most naturalists and geologists reason in the same way,—some more cautiously than others,—yet perhaps most of them seem not to perceive how far such inferences imply the doctrine of the common origin of related species.

For obvious reasons such doctrines are likely to find more favor with botanists than with zoologists. But with both the advance in this direction is seen to have been rapid and great; yet to us not unexpected. We note, also, an evident disposition, notwithstanding some endeavors to the contrary, to allow derivative hypotheses to stand or fall upon their own merits,—to have indeed upon philosophical grounds certain presumptions in their favor,—and to be, perhaps, quite as capable of being turned to good account as to bad account in natural theology.[3]

[3] What the Rev. Principal Tulloch remarks in respect to the philosophy of miracles has a pertinent application here. We quote at second hand:

"The stoutest advocates of interference can mean nothing more than that the Supreme Will has so moved the hidden springs of nature that a new issue arises on given circumstances. The ordinary issue is supplanted by a higher issue. The essential facts before us are a certain set of phenomena, and a Higher Will moving them. How moving them? is a question for human definition; the answer to which does not and cannot affect the Divine meaning of the change. Yet when we reflect that this Higher Will is everywhere reason and wisdom, it seems a juster as well as a more comprehensive view to regard it as operating by subordination and evolution, rather than by interference or violation."

Among the leading naturalists, indeed, such views—taken in the widest sense—have one and, so far as we are now aware, only one thoroughgoing and thoroughly consistent opponent, viz.: Mr. Agassiz.

Most naturalists take into their very conception of a species, explicitly or by implication, the notion of a material connection resulting from the descent of the individuals composing it from a common stock, of local origin. Mr. Agassiz wholly eliminates community of descent from his idea of species, and even conceives a species to have been as numerous in individuals and as wide spread over space, or as segregated in discontinuous spaces, from the first as at a later period.

The station which it inhabits, therefore, is with other naturalists in no wise essential to the species, and may not have been the region of its origin. In Mr. Agassiz's view the habitat is supposed to mark the origin, and to be a part of the character of the species. The habitat is not merely the place where it is, but a part of what it is.

Most naturalists recognize varieties of species; and many, like DeCandolle, have come to conclude that varieties of the highest grade, or races, so far partake of the characteristics of species, and are so far governed by the same laws, that it is often very difficult to draw a clear and certain distinction between the two. Mr. Agassiz will not allow that varieties or races exist in nature, apart from man's agency.

Most naturalists believe that the origin of species is supernatural, their dispersion or particular geographical area, natural, and their extinction, when they disappear, also the result of physical causes. In the view of Mr. Agassiz, if rightly understood, all three are equally independent of physical cause and effect, are equally supernatural.

In comparing preceding periods with the present and with each other, most naturalists and palæontologists now appear to recognize a certain number of species as having survived from one epoch to the next, or even through more than one formation, especially from the Tertiary into the Post-tertiary period, and from that to the present age. Mr. Agassiz is understood to believe in total extinctions and total new creations at each successive epoch, and even to recognize no existing species as ever cotemporary with extinct ones, except in the case of recent exterminations.

These peculiar views, if sustained, will effectually dispose of every form of derivative hypothesis.

Returning for a moment to DeCandolle's article, we are disposed to notice his criticism of Linnæus's 'definition' of the term *species* (*Phil. Bot.*, No. 157): *Species tot numeramus quot diversæ formæ in principio sunt creatæ*,—which he declares illogical, inapplicable, and the worst that has been propounded. "So, to determine if a form is specific, it is necessary to go back to its origin, which is impossible. A definition by a character which can never be verified is no definition at all."

Now, as Linnæus practically applied the idea of species with a sagacity which has never been surpassed, and rarely equalled, and indeed may be said to have fixed its received meaning in natural history, it may well be inferred that in the phrase above-cited he did not so much undertake to frame a logical *definition*, as to set forth the *idea* which, in his opinion, lay at the foundation of species. On which basis A. L. Jussieu did construct a logical definition:—"nunc rectius definitur perennis individuorum

similium successio continuata generatione renascentium." The fundamental idea of species, we would still maintain, is that of a chain, of which genetically-connected individuals are the links. That, in the practical recognition of species, the essential characteristic has to be *inferred*, is no great objection,—the general fact that like engenders like being an induction from a vast number of instances, and the only assumption being that of the uniformity of nature. The idea of gravitation, that of the atomic constitution of matter, and the like, equally have to be verified inferentially. If we still hold to the idea of Linnæus, and of Agassiz, that existing species were created independently, and essentially all at once at the beginning of the present era, we could not better the propositions of Linnæus and of Jussieu. If, on the other hand, the time has come in which we may accept, with DeCandolle, their successive origination, at the commencement of the present era or before, and even by derivation from other forms, then the '*in principio*' of Linnæus will refer to that time, whenever it was, and his proposition be as sound and wise as ever.

In his *Géographie Botanique* (ii, 1068–1077) DeCandolle discusses this subject at length, and in the same interest. Remarking that of the two great facts of species, viz: *likeness among the individuals*, and *genealogical connection*, zoologists have generally preferred the latter,[4] while botanists have been divided in opinion, he pronounces for the former as the essential thing, in the following argumentative statement:

"Quant à moi, j'ai été conduit, dans ma definition de l'espèce, à mettre décidément la ressemblance au-dessus des caractères de succession. Ce n'est pas seulement à cause des circonstances propres au règne végétal, dont je m'occupe exclusivement; ce n'est pas non plus afin de sortir ma définition des théories et de la rendre le plus possible utile aux naturalistes descripteurs et nomenclateurs, c'est aussi par un motif philosophique. En toute chose il faut aller au fond des questions, quand on le peut. Or, pourquoi la reproduction est-elle possible, habituelle, féconde indefiniment, entre des êtres organisés que nous dirons de la même espèce? Parce qu'ils se ressemblent et uniquement à cause de cela. Lorsque deux especes ne peuvent, ou, s'il s'agit d'animaux supérieurs, ne peuvent et ne veulent se croiser, c'est qu'elles sont très differentes. Si l'on obtient des croisements, c'est que les individus sont analogues; si ces croisements donnent des produits féconds, c'est que les individus étaient plus analogues; si ces produits eux-mêmes sont féconds, c'est que la ressemblance était plus grande; s'ils sont fécond habituellement et indéfiniment, c'est que la ressemblance intérieure et extérieure était très grande. Ainsi le degré de ressemblance est le fond; la reproduction en est seulement la manifestation et la mesure, et il est logique de placer la cause au-dessus de l'effet."

We are not at all convinced. We still hold that genealogical connection, rather than mutual resemblance is the fundamental thing,—first on the ground of fact, and then from the philosophy of the case. Practically, no botanist can say what amount of dissimilarity is compatible with unity of species; in wild plants it is sometimes very great, in cultivated races, often enormous. DeCandolle himself informs us that the different variations which the same Oak tree exhibits are significant indications of a disposition to set up separate varieties, which becoming hereditary may

[4] Particularly citing Flourens: "La ressemblance n'est qu' une condition secondaire; la condition essentielle est la descendance: ce n'est pas la ressemblance, c'est la succession des individus, qui fait l'espèce."

constitute a race; he evidently looks upon the extreme forms, say of *Quercus Robur*, as having thus originated; and on this ground, inferred from transitional forms, and not from their mutual resemblance, as we suppose, he includes them in that species. This will be more apparent should the discovery of the transitions, which he leads us to expect, hereafter cause the four provisional species which attend *Q. Robur* to be merged in that species. It may rightly be replied that this conclusion would be arrived at from the likeness step by step in the series of forms; but the cause of the likeness here is obvious. And this brings in our '*motif philosophique*.'

Not to insist that the likeness is after all the variable, not the constant, element,—to learn which is the essential thing, resemblance among the individuals or their genetic connection, we have only to ask which can be the cause of the other.

In hermaphrodite plants (the normal case), and even as the question is ingeniously put by DeCandolle in the above extract, the former surely cannot be the *cause* of the latter, though it may, in case of crossing, offer *occasion*. But, on the ground of the most fundamental of all things in the constitution of plants and animals, "the fact incapable of farther analysis, that individuals reproduce their like, that characteristics are inheritable,"[*] the likeness is a direct natural consequence of the genetic succession,—and it is logical to place the cause above the effect.

We are equally disposed to combat a proposition of DeCandolle's about genera, elaborately argued in the *Géographie Botanique*, and incidentally re-affirmed in his present article, viz., that genera are more natural than species, and are more correctly distinguished by people in general, as is shown by vernacular names. But we have no space left in which to present some evidence to the contrary.

Here we must abruptly close our long exposition of a paper which, from the scientific position, ability, and impartiality of its author, is likely at this time to produce a marked impression. We would also direct attention to an earlier article in the same important periodical (viz: in the *Bibl. Univ.* for May, 1862), on the European Flora and the Configuration of Continents in the Tertiary Epoch, a most interesting abstract of, and commentary on, the introductory part of Heer's *Flora Tertiaria Helvetiæ*, as reëdited and translated into French by Gaudin, with additions by the author. A. G.

3. *Flora Capensis;* by Dr. HARVEY and Dr. SONDER; vol. ii, 1861–62. The second volume of this excellent work extends from the *Leguminosæ* to the *Loramthaceæ* inclusive, that is, it concludes the Polypetalous orders. Almost half the volume is devoted to the *Leguminosæ*, elaborated by Dr. Harvey, and much the greater part of the other half is occupied by the *Bruniaceæ*, by Dr. Sonder (who assigns no definite character to separate them from *Hamamelideæ*), the *Crassulaceæ*, by Dr. Harvey, the *Mesembryaceæ* by Dr. Sonder (*Mesembryanthemum* counting 300 species, including 7 not sufficiently known), and the *Umbelliferæ*, by Dr. Sonder. *Montinia* is transferred by Dr. Harvey from the *Onagraceæ* to the *Saxifragaceæ*. The close affinity of the latter order to *Rosaceæ* is recognized by placing it and its immediate allies next after *Rosaceæ* in the series.

A. G.

[*] See this Journal, vol. xxix, [2], March, 1860, p. 165, for the enunciation of this obvious principle.

4. *Flora of Canada.—Flore Canadienne, ou Descriptions de toutes les Plantes des Forêts, Champs, Jardins et Eaux du Canada*, &c.—Par l'Abbé L. PROVANCHER, Curé de Portneuf. Quebec: Joseph Darveau, 1862. 2 vols, 8vo. pp. 842.—It is pleasant to find that Botany is attracting so much attention in Lower Canada as to call into existence a Canadian Flora in the French language; and it is much to the credit of the Abbé Provancher, for zeal and enterprize, that he should have produced such a work as this, in so good a form and so neatly printed. It is of course substantially a compilation; and the author is evidently a neophyte, of limited acquaintance with the plants around him; but he makes a fair beginning, in a work which may for the present very well serve the educational end in view. The critical Flora of Canada and the other Provinces is yet to be written, and will be of a different order.

The wood cuts, "over 400 in number," which illustrate the orders, and which here appear in such novel guise with their French environment, are every one taken from Gray's Botanical Text Book, except five of the Ferns from the Manual,—a preference which speaks more for the good taste of the Abbé than does the omission to mention the source.

A. G.

5. *The Tendrils of Virginia Creeper terminating in flat expansions or disks*, by means of which this climber readily ascends smooth trunks and walls, appear to have attracted Mr. Des Moulin's attention, at Bordeaux, as a great curiosity. They are described at length by him in the *Transactions of the Linnæan Society* of that city. Before publishing, however, he had become aware that this peculiarity was described in the *Manual of Botany of Northern States* in 1856. We can give him earlier dates; i. e., *Torrey & Gray, Flora of N. America*, i, 245 (1838); and the venerable Dr. Darlington's *Flora Cestrica*, 2d ed., p. 153 (1837). Probably there is still earlier mention of it; as the fact has been familiar to us from boyhood. These disks are figured in *First Lessons in Botany*, p. 38. We may add that on the same plant may often be seen these disk-bearing tendrils and others which act in the ordinary manner. Although we have never seen aërial rootlets also, to verify the character "caule radicando-scandente" in Michaux, yet these are mentioned by Dr. Darlington, who is generally very correct, and are not unlikely to appear under favorable conditions, as they do in the Southern Muscadine Grape. A. G.

6. *Vites Boreali-Americanæ*, par. E. DURAND, *de l'Académie des Sciences Naturelles de Philadelphie*, etc. *Memoire précédé d'une Introduction* par M. CH. DES MOULINS, etc.—In response to demands from the French Society for Acclimatisation, and from Mr. Des Moulins on the part of the naturalists and vine-growers of Bordeaux, the excellent Mr. Durand of Philadelphia, along with other practical information, communicated a condensed but very careful monograph of the North American species of Vitis. This monograph,—a most laudable attempt to illustrate an extremely difficult group of species,—is published in the *Actes de la Société Linnéene de Bordeaux*, vol. xxiv, issued at the close of the last year, greatly amplified in bulk by the garrulous introduction, intercalations and notes of its French editor. Seven pages of this introduction are devoted mainly to a criticism of the two words by which the present

writer distinguished the genus *Ampelopsis*, viz.: "disk none." The substance of the whole is, that Mr. Des Moulins admits that no disk is to be found in the flower of *Ampelopsis*, but thinks that he finds under the forming fruit something which, if it developed, would become a disk: then stating, in effect, that the disk in *Ampelideæ* is nothing more than a development of the common receptacle of the flower (to which we have no present occasion to object), he insists that this disk equally exists "*plus ou moins fort*," in *Ampelopsis* where it is not *developed* at all. A reëxamination enables us to say that Mr. Sprague's figures in the *Genera Am. Bor. Ill.*, ii, pl. 162 are correct, and that there is no disk at all developed in *Ampelopsis*. Such are the facts. If now it be argued that this genus should be united to *Vitis* in spite of this difference, we could not well object, knowing how variable the disk is in different species of *Vitis* (including *Cissus*), and that a Brazilian species of the latter is hardly distinguishable from our Virginia Creeper except in its strongly developed disk. Bentham and Hooker fil., we observe, have recently made this reduction; but still upon an unfounded hypothetical basis. They write: "*Ampelopsis* .... exhibet discum cum ovario omnino confluentem;" —a view which we can no more confirm by observation than we can that of Des Moulins; but it has the immense advantage of being stated in fewer words than the latter requires of pages. A. G.

7. *Vegetable Productions of the Feejee Islands.*—A "Blue Book," entitled "*Correspondence relative to the Fiji Islands*," May, 1862, gives a full and official account of the arrangement between the British Consul, Mr. Pritchard, and *Ebenezer Thakombau*, claiming to be king of the Fiji Islands, for the cession of the latter to the British crown, and of the appointment of Col. Smythe as a commission to visit these islands and to report whether the acquisition would be desirable,—whereupon the commissioner visited the islands, accompanied by Dr. B. Seemann, who was instructed to explore and report upon their vegetable productions and resources. Col. Smythe very sensibly reported that Thakombua, although perhaps the most influential of the independent chiefs, had no claim to the title of king of Fiji, and that it was inexpedient to accept his offer. What most interests us is the appendix, containing Dr. Seemann's elaborate Report on the Vegetable Productions and Resources of the Vitian or Fijian Islands. This treats, 1, of the climate, soil, and flora in general of these islands, and, 2, of the Colonial Produce, so-called, such as sugar, coffee, tamarinds and tobacco, which they may be expected to yield, as also certain oils and fats, farinas, and spices. 3. The staple food of the people. This "is the same all over Polynesia, being derived, with the total exclusion of all grain and pulse, from the yam, the taro, the banana, the plantain, the bread fruit, and the cocoa-nut; but the bulk of it is furnished in the different countries by only one of these plants. In the Hawaian group the taro takes the lead, whilst the cocoa-nut is looked upon as a delicacy, from which the women were formerly altogether cut off. In some of the smaller coral islands the inhabitants live almost entirely upon cocoa-nuts. The Samoans place the bread-fruit at the head of the list. Again, the Fijians think more of the yam than of the others, though all grow in their islands in the greatest perfection, and in an endless number of varieties." Of edible fruits there is a long list, the bread-fruit

and bananas being the most important, and the account of the *Ivi*, (*Inocarpus edulis*), is the most interesting, now that its botanical relationship has been detected by Mr. Bentham. 4. *Cannibal vegetables*, the vegetables eaten with human flesh,—formerly an important part of Fijian dieteties, and not yet entirely obsolete,—form the subject of a separate section. Human flesh, it appears, is extremely difficult to digest, and, perhaps on this account, was eaten with the leaves of three vegetables which were thought to assist the process, viz: of *Trophis anthropophagorum* and *Solanum anthropophagorum* of Seemann, and of *Omalanthus pedicellatus*, Benth., an Euphorbiaceous plant. 5. *National Beverages.* Like the other Polynesians, they prepare an intoxicating drink from the root of *Piper methysticum.* "In order to prepare the beverage, it is necessary to reduce the roots to minute particles, which, according to regular Polynesian usage, is done by chewing,—a task, in Fiji, devolving upon lads who have sound teeth, and who occupy a certain social rank towards the man for whom they perform the office.... Some Fijians make it a point to chew as great a quantity as possible in one mouthful; and there is a man of this sort at Veratra, famous all over the group, who is able within three hours' time to chew a single mouthful sufficient to intoxicate fifty persons." Although the Fijians drink the natural liquor of young cocoa-nuts, they were not acquainted, nor were any Polynesians acquainted, with the art of extracting and fermenting toddy from the cocoa-nut palm. From which it is inferred, that, if the Polynesians are of Malayan origin, they must have left the cradle of their race before the extraction of toddy from the cocoa-nut tree, or even the tree itself, was known there. Indeed, this palm itself is thought to have made its way by the drifting of its fruits across the Pacific from east to west, through the Polynesian Islands, and to have reached Ceylon within what may be called historical times. 6. *Vegetable Poisons.* Under this head is an interesting account of the kau-karo (literally Itch-wood), the *Oncocarpus Vitiensis* A. Gray, which acts like the Poison Rhus of North America and of Japan, only with tenfold virulence. Indeed, a drop of the juice, falling upon the hand of one of Dr. Seemann's companions, "instantly produced a pain equal to that produced by contact with a red-hot poker." The *Excœcaria Agallocha*, known through the East, is equally virulent with its ally the Manchineel tree. The smoke of the burning wood is used by the Fijians to cure leprosy,—a terribly severe, but sometimes an effectual, remedy. 7. *Medicinal Plants.* None of real importance are brought to light. 8. *Scents and Perfumes.* These are used for scenting the cocoa-nut oil which the natives profusely apply to the hair and to the naked body. Besides that obtained from several flowers, from the fruit of *Parinarium laurinum* and of *Eugenia* (*Jambosa*) *neurocalyx* A. Gray, and from the bark of a species of *Cinnamomum*, the most famous is that yielded by the Sandalwood of the islands, which, formerly abundant at Sandalwood Bay, is now almost annihilated. 9. *Materials for Clothing.* The *tapa*, made of the bark of the Paper Mulberry, mainly furnished what scanty clothing was needed, until the introduction of cheap cotton cloth by traders. Successive sections discourse of *Fibres used for cordage;* of *Cotton*, several sorts of which have been introduced and run wild in these islands, and the better sorts are now cultivated with success; of *Timber*, the most

important being a kind of kowrie-pine or *Dammara*, and *Calophyllum inophyllum*. The wood of the latter, abundant by the sea-side, is used for canoes and boats, while its seed yields an important oil, but the most valued wood is that of *Afzelia bijuga* A. Gr., which is almost indestructible. *Palms, Sacred Groves, Ornamental Plants*, &c., occupy the remaining sections.

Having submitted his economical report, Dr. Seemann is now turning his attention to the scientific botany of the Feejee Islands, where he made a collection second in extent and interest only to that of the U. S. Exploring Expedition under Commodore Wilkes. The *Flora Vitiensis* which he has announced as in preparation, is to be a royal quarto volume of about 400 pages of letter press, and 100 colored plates by Mr. Fitch,—to be published by Lovell Reeve and Co. In form and extent it will therefore equal his well known Botany of the Voyage of the Herald; and it can not fail to be interesting and important.

A *Synopsis Plantarum Vitiensium*, or List of the Fijian Plants at present known, has just been issued by Dr. Seemann, corrected up to date. We note that he has overlooked Mr. Sullivant's folio, of the *Musci* of Wilkes' Expedition, in which fifteen mosses not in his list are enumerated or described from these islands, and six of them are figured. The *Lichenes* by Mr. Tuckerman, the *Algæ* by Prof. Harvey and the late Prof. Bailey, and the few *Fungi*, by Messrs. Curtis and Berkeley, also published, but sparingly diffused, may also add something to the list. A. G.

8. *New Edition of Gray's Manual of the Botany of the Northern United States.*—We copy the *Advertisement to the revised edition*, 1863. —"The additions and alterations of the Revised Edition of this work, now issued, are mainly the following:

"1. The addition of an entirely new part, entitled GARDEN BOTANY, AN INTRODUCTION TO A KNOWLEDGE OF THE COMMON CULTIVATED PLANTS: see pp. xxix–lxxxix. By this, the common exotics, no less than the wild plants, are made available for botanical classes, which will be a great convenience in many cases. Most of these cultivated plants are everywhere common, and generally at hand for botanical illustration; and it is desirable that they should be scientifically known and rightly named. And there is no great difficulty in studying them, if *double* flowers, and those which are otherwise in a monstrous or unnatural condition, be avoided, at least by beginners. It is obviously absurd and highly inconvenient to mix in the cultivated with the wild plants in such a work as this. But a separate account of the common exotics, annexed and subsidiary to the *Botany of the Northern United States*, especially in the School Edition, will doubtless be popular and useful. Directions for the use of the *Garden Botany* will be found on p. xvii and p. xxix.

"2. The ANALYTICAL KEY, p. xvii, upon which the pupil so greatly depends, has been altogether revised, much simplified, adapted to the *Garden Botany* as well as to the *Botany of the Northern States*, and printed in a larger type.

"3. Numerous corrections in particulars have been made throughout the body of the work, whenever the required alterations could well be effected upon the stereotype plates. Many others, suggested by acute and obliging correspondents, or by my own observation, are necessarily deferred until the work can be recomposed.

"4. The plants which have been newly detected within our limits, and one or two which were before accidentally omitted, are enumerated and characterized in the ADDENDA, p. xc.

"5. Eight plates have been added, crowded with figures, illustrating all the genera (66 in number) of Grasses. They are wholly original, having been drawn from nature and engraved by Mr. Sprague. They will be of great assistance in the study of this large, difficult, and important family.

"The flattering success which the *Manual* has met with stimulates the author's endeavors towards its continued improvement;—in regard to which he still solicits aid from his correspondents."

9. *Botanical Necrology*, 1862.—Of the three botanists of Holland who all died in the earlier weeks of the year 1862, viz.: *Blume*, *Van den Bosch*, and *De Vriese*, a brief record was made in this Journal for May last.

Prof. M. N. Blytt, of the University of Christiania, the most distinguished Norwegian botanist, died on the 26th of July last, aged 70 years. He had amassed vast materials for the illustration of Scandinavian botany, and had commenced the publication of his *Norges Flora*, the first volume of which appeared in 1861.

Wm. Borrer, Esq., of Henfield in Sussex, England, one of the venerable cotemporaries and botanical friends of Sir James E. Smith, and whose name has long been intimately associated with English botany, died on the 10th of February, 1862, in the 81st year of his age.

Dr. James Townshend Mackay, the author of the *Flora Hibernica*, long the director of the Botanic Garden of Trinity College, Dublin, died five days later, viz: on the 15th of February, at an age little less venerable than that attained by Mr. Borrer.

Dr. D. G. von Kieser, the late President of the Imperial German Society of Naturalists, and who has been Professor of Medicine at the University of Jena ever since 1812, died on the 11th of October, aged 83 years. He is to be honorably mentioned among the botanists, on account of two early essays on the anatomy and physiology of plants, one of which, in the year 1812, took the prize offered by the Haarlem Academy; and for his *Elements of the Anatomy of Plants*, the earliest German treatise of modern times, published in 1815.

Dr. Joachim Steetz, of Hamburg, died on the 24th of March, 1862, in the 57th year of his age. He was a medical practitioner, who devoted his leisure hours with assiduity and much success to systematic botany, and especially, in his later years, to the *Compositæ*.

Mr. John Tweedie, a Scotch gardener, who visited Buenos Ayres to make botanical collections on the LaPlata, the Parana, and the Uruguay, &c., more than thirty years ago, and became so fond of the country that he made it his home, died at Santa Catalina, near Buenos Ayres, on the first of April, 1862, at the age of 87. To him we are mainly indebted for the original of the Verbenas which adorn our parterres, and for many other ornamental cultivated plants.

Turning now to our home circle, we have to record the honored names of four of the older cultivators of our science who have been removed from our thin ranks within the last few months:—

Benjamin D. Greene, Esq., of Boston, died on the 14th of October last, at the age of 69 years. He was born in 1793, was graduated at Harvard University in the year 1812; he first pursued legal studies, partly in the then celebrated school at Litchfield, Connecticut, and was duly admitted to the Bar in Boston. He then took up the study of

medicine, and completed his medical course in the schools of Scotland and Paris, taking his medical degree at Edinburgh in the year 1821. The large advantages of such a training having been enjoyed, Mr. Greene did not engage in the practice of either profession. An ample inheritance, which rendered professional exertion unnecessary, conspiring with a remarkably quiet and contemplative disposition, and a refined taste, led him to devote his time to literary culture and to scientific pursuits. His fondness for botany, which early developed, was stimulated by personal intercourse with various European botanists, and especially with his surviving friend, the now venerable Sir Wm. Hooker, then Professor in the University of Glasgow, to whom he naturally became much attached, and by whom he was highly appreciated.

In botany, as in everything else, Mr. Greene sought to be silently useful. He never himself published any of his discoveries or observations. The few species to which his name is annexed were given to the world at second-hand. But his collections were extensive, his original observations numerous and accurate, and both were freely placed at the disposal of working botanists. He early saw that the great obstacles to the advantageous prosecution of botanical investigations in this country, and especially in New England, were the want of books and the want of authentic collections; and these desiderata he endeavored, so far as he could, to supply. He gathered a choice botanical library, he encouraged explorations, and he subscribed to all the large purchasable North American collections,—beginning with those of Drummond in the Southern United States and in the then Mexican province of Texas. These, being distributed under numbers, among the principal herbaria of the world, and named or referred to in monographs or other botanical works, were of prime importance as standards of comparison. Such collections and such books as Mr. Greene brought together were just the apparatus most needed at that time in this country; and now, when our wants are somewhat better supplied, we should not forget the essential service which they have rendered, nor the disinterested kindness with which their most amiable and excellent owner always placed them at the disposal of those who could advantageously use them. Mr. Greene's botanical library and collections have been, by gift and by bequest, consigned to the Boston Society of Natural History, of which he was one of the founders and the first President,—and by which they will be preserved for the benefit of future New England botanists, by whom his memory should ever be gratefully cherished. The genus *Greenea*, established by Wight and Arnott upon two rare Rubiaceous shrubs of India, barely anticipated a similar dedication by his old friend Mr. Nuttall, of a curious Grass of Arkansas and Texas, and will perpetuate his name in the annals of the science which he lovingly cultivated.

Dr. Asahel Clapp, of New Albany, Indiana, died on the 17th of December last, as has already been announced in the current volume of this Journal (p. 306). We are not informed of the particulars of his life, nor of his exact age, but we suppose he had nearly or quite reached his three-score years and ten. His only botanical publication is one of merit and importance, viz., *A Synopsis or Systematic Catalogue of the Medicinal Plants of the United States*, which forms an 8vo volume of

222 pages. It was presented to the American Medical Association in May, 1852, and published during that year, at Philadelphia. A rare plant of the order *Compositæ*, which inhabits the southern borders of Texas, was dedicated to Dr. Clapp in the Botany of the Mexican Boundary Survey.

Dr. Melines C. Leavenworth, as already announced in this Journal, died in the vicinity of New Orleans, in December last, while acting as Surgeon to the 12th Connecticut regiment, at the age of probably above three score years. It is to be desired that some one acquainted with them would put upon record the incidents of his life. He was formerly and for many years a surgeon in the United States Army; from which, however, he retired about twenty years ago. While in the army, and at frontier posts in Arkansas, Louisiana, and Florida, he indulged his strong botanical tastes, and did useful service, by observing and collecting the plants within his reach, which he communicated to Dr. Torrey along with copious notes. These were the more important as his dried specimens were seldom neatly preserved. The pages of the *Flora of North America*, upon which his name so often occurs, testify to his zeal and success as a botanical explorer and pioneer. His ardent love of botany—fostered, we believe, by the late Dr. Tully—must have early developed; for as much as forty years ago he discovered "four new plants from Alabama," which he described in the seventh volume of this Journal, in 1824. Among the many rare plants which he detected, a very peculiar one—the *Amphianthus pusillus* of Torrey—which he found in the upper part of Georgia, is so very scarce and local that it has never been met with since. A pretty and strikingly marked Cruciferous genus, one species of which (if indeed distinct from the other) was discovered by Dr. Leavenworth, dedicated to him by Dr. Torrey, commemorates his botanical services;—which services, indeed, were continued to the last. For no sooner had he landed with his regiment upon our southern coast than he zealously began to collect the plants he met with, and to note their peculiarities. Although his scientific acquirements and insight were not great, his zeal and devotion to botany were thorough and genuine. A. G.

Dr. Charles Wilkins Short died at Louisville, Ky., March 7, aged 69 years. A notice of his life will appear in our next issue.

Zoology—

10. *Evidence as to Man's place in Nature;* by Thomas Henry Huxley, Fellow of the Royal Society. 160 pp. 8vo. London: Williams and Norgate.—The able zoologist, Prof. Huxley, discusses in the first chapter of his work, "The Natural History of the Man-like Apes," or the Orangs, Gibbons, Gorillas and Chimpanzees; in the second, "the Relations of Man to the lower animals;" and in the third, the "Fossil remains of Man." The second topic is that towards which all the rest of the work points; and the conclusion of the whole is, that man belongs structurally to the same order with the *Quadrumana*, and constitutes among the *Primates* (as the order is called, after Linnæus), the family of *Anthropini;* and further, that "if man be separated by no greater structural barrier from the brutes than they are from one another, then it seems to follow that if any process of physical causation can be discovered, by which the genera and families of ordinary animals have been

produced, that process of causation is amply sufficient to account for the origin of Man;" and, finally, that the theory of Mr. Darwin is "the only one that has any scientific existence," and is, probably, the true one, or at least, "if not precisely true, the hypothesis is as near an approximation to the truth as, for example, the Copernican hypothesis was to the true theory of the planetary motions" (p. 107).

The main argument of the work has been met by the writer in his article (this volume, p. 65), on the Classification of Mammals. It is there shown, that Man stands apart from all other Mammals, on the basis of a characteristic of profound zoological value. The characteristic referred to is this:—that, in Man, the fore-limbs are withdrawn completely from the *locomotive* series, and transferred to the *cephalic;* and, thus, a very large anterior portion of the body is turned over to the service of the head, while the posterior or gastric portion is reduced to its minimum. This condition of extreme *cephalization* in the system is of the very highest significance, and places Man alone. Man's erect structure is a part of its expression. The nature of the feet in Man,—they being made simply for supporting the body, and not, as in the Quadrumana, for clinging or grasping—is a concomitant feature of his erectness; and such also is the position of the cerebellum wholly beneath the cerebrum, mentioned in Professor Owen's characteristics of Man. For the argument on the subject, we refer to the article mentioned.

The uses of the fore-limbs in man are, first, the *inferior*, depending on the demands of the appetite satisfied through the mouth (uses that are united to the locomotive in the Apes and some other quadrupeds); second, the *superior*, depending on the demands of Man's higher nature.

This higher nature, it may be added, we regard as a spiritual one, in which the brute has no share, and to the possession of which no development-process could elevate him. The raising of the fore-limbs from the ground, for esthetic, intellectual, and spiritual service, was in direct harmony with such a spiritual endowment. Man exhibits his exclusive possession of such an element, not merely in having the power of speech, but more fundamentally in being the only species capable of reaching towards a knowledge of himself, of nature, and of God;—the only one, therefore, capable of conscious obedience, or disobedience, of any moral law, and the only one subject to degradation through the appetites and a moral nature. His power of indefinite progress, his thoughts and desires that look onward even beyond time, his recognition of spiritual existence and of a Divinity above, all evince a nature that partakes of the infinite and divine. Man is linked to the *past* through the system of *life*, of which he is the last, the completing, creation. But, unlike other species of that closing system of the *past* (significantly the *Zoic* era of geological history), he, through his *spiritual* nature, is far more intimately connected with the opening *future*.

Whatever the point of view, then, we see reason wholly to dissent from the sentiment with which Prof. Huxley concludes his chapter "on the relations of Man to the lower animals" (p. 112): "Our reverence for the nobility of manhood will not be lessened by the knowledge, that Man is, in substance and in structure, one with the brutes; for he alone possesses the marvellous endowment of intelligible and rational speech, whereby, in the secular period of his existence, he has slowly accumu-

lated and organized the experience which is almost wholly lost with the cessation of every individual life in other animals; so that now he stands raised upon it, as on a mountain top, far above the level of his humble fellows, and transfigured from his grosser nature by reflecting, here and there, a ray from the infinite source of truth." It is possible to conceive that a being with such mental endowments as Man possesses, and with even the throat of a gorilla, might originate an intelligible language; but it is incomprehensible how the gift of speech could develop man's mental qualities in a brute, however long the time allowed. Moreover, it is a natural question, why there are not Man-apes in the present age of the world, representing the various stages of transition, and filling up the *hiatus*, admitted to be large, if such a process of development is part of the general system of nature. We think this question a fair one, notwithstanding the reply which may be made, that the more developable individuals long since passed out of the Ape-stage, leaving behind only the unimprovable ones. The resemblances between the skeletons of Man and the Apes, and between ova generally, mentioned by Prof. Huxley, may, to the uninitiated in science, appear to make the transition by development feasible: yet they are of no weight as argument, since the question is as to the *fact* whether, under nature's laws, such a transition has taken place as the gradual change of an Ape into a Man, or, whether Apes were made to be, and remain, Apes. In the Ape, the great muscle of the foot, the *flexor longus pollicis*, divides and sends a branch to three or more of the toes, while in Man, it passes to the great toe alone: Is it a *fact* that this, and the many structural differences of the foot and other parts of the body, were brought about by gradual development in a progressive Ape?

Between the lowest and highest types of men, there are all possible intermediate shadings as to grades of intellect, size of brain, and form of features. The range of grades, thus passing into one another through small individual differences, is very wide among the several diverse tribes of negroes in Africa; it is very wide in the present population of Europe, and even in Britain alone. For these and other reasons, we may believe in the unity of origin of the human race. But with regard to Man and the Man-apes, no evidence has been pointed out, derived from Man, or the Apes, proving either the fact, or the probability, or the possibility, of a common origin. The direct evidence, on which the Darwinian hypothesis rests, comes from lower departments of life, and is acknowledged by its advocates to be exceedingly scanty and imperfect: they would say—and rightly—that facts have but just begun to be collected. But on this general subject, it is not our purpose now to enter.

The few discoveries of ancient skulls of inferior capacity, made recently in Europe, indicate the condition of some of the early tribes on *that* continent, or, at least, of some individuals in those tribes, and are of great archæological interest. The skulls are not inferior to those of some of the lowest of living men; and Prof. Huxley remarks respecting them, that they do not seem to him "to take us appreciably nearer to the lower pithecoid form," that is, to that of the Man-apes.

Should similar discoveries be made all over the globe, proving a

former condition of the race much inferior to the present, such a phase in its history is one that would have been a necessary consequence of man's nature, however large his skull or brain when created. For, as Prof. Guyot has observed, a first race, very limited in language and knowledge, without arts, and possessing, in full force, man's natural selfishness, unbridled appetites, and evil propensities, would have entered at once upon a course of degradation; and, before many centuries had passed, the whole population, unless some part or all had been restrained or guided by superhuman agency, would have sunk to its lowest limit of moral and physical debasement. It is not too much to expect that the fact of a general physical debasement may be ultimately proved by the discoveries now in progress. J. D. D.

11. *On the question whether Diatoms live on the sea-bottom at great depths;* by WM. STIMPSON, M.D.—In a paper on the Diatomaceæ found in mud collected at great depths from the bottom of the sea off the coast of Kamtschatka, in soundings made by the North Pacific Expedition under Com. Rodgers (see *this Journal*, [2], xxi, 284), the late lamented Professor Bailey made the following remark. "The perfect conditions of the organisms in these soundings, and the fact that some of them retain their soft parts, indicate that they were very recently in a living condition, but it does not follow that they were living when collected at such immense depths." My attention has recently been called to this subject by the perusal of an account of the recent discoveries of animal life in various forms at depths vastly greater than had been previously suspected; for instance, at 1400 fathoms by Torell, at 1000 and 1500 fathoms by Milne-Edwards, and at 3000 fathoms by Dr. Wallich. The question of the nature of the food of these abysmal animals is one of great interest, and I wish to place on record, in advance of the publication of the report of the expedition, the results of my examination of the specimens alluded to by Prof. Bailey, when they were freshly taken from the water.

In the sounding taken at the depth of 2700 fathoms, in lat. 56° 46′ N., long. 168° 18′ E., Lieut. Brooke used, for the armature of his lead, three quills, each about three inches in length, fastened together, and placed in such a position that when the lead struck the bottom the quills would be forced perpendicularly into it, and thus become filled with mud from a stratum a few inches below the general surface of the sea-bottom. The experiment was successful; the quills coming up compactly filled with mud of the usual character occurring at such depths in such latitudes. One of the quills having been submitted to me for microscopic examination, was carefully wiped and cut in two at the *middle*, in order to secure for examination a specimen, as nearly as possible free from any chance admixture from the water near the surface. In this specimen I found an abundance of diatoms, some of which, apparently *Coscinodisci*, appeared to me to be undoubtedly living, judging from their fresh appearance and the colors of their internal cell contents.

It is exceedingly doubtful whether sufficient light can penetrate to so great a depth to afford the stimulus which these vegetable organisms are suppose to require for their existence and multiplication. On the other hand, it is by no means certain that some amount of light does not so penetrate, and, if we deny the existence of vegetable life in these

abysses, it will be difficult to account for the existence there of animals, which must, ultimately, derive their sustenance from the vegetable kingdom. The supply which they might obtain from the dead bodies of those organisms which die at the surface, and slowly sink through two or three miles of water to the bottom, seems totally insufficient, for Dr. Wallich has proved that the animals, starfishes for instance, not only exist at those depths, but exist in great numbers. We would call the attention of those who may have an opportunity of obtaining specimens of the bottom at great depths, to the great importance of a microscopic examination of these specimens as soon as taken from the sea. Fresh water should, of course, be used in spreading the mud upon the slide.

12. *On the "genus Diplothyra."*—Having received from Mr. Sanderson Smith, of New York, a fine series of the shell recently described by Mr. Tryon in the *Proceedings of the Philadelphia Academy*, as a new genus and species of Pholadidæ, "*Diplothyra Smithii*," I have satisfied myself that Mr. Tryon is wrong in considering the accessory valve as double, and that the shell in question is a true *Martesia*. It is, in fact, very closely allied to *M. cuneiformis*, which often presents an accessory valve of precisely the same character. WM. STIMPSON.

13. *On Part II. of Prof. G. Jan's Prodromo della Iconografia Generale degli Ofidi;* by E. D. COPE.—Among the constantly appearing contributions to Herpetology, few are more valuable than those upon the serpents, issued by Prof. G. JAN, director of the Museum at Milan. This value is however dependent rather upon the number of new forms made known, and the beautiful plates illustrating the work, than upon unusual merit in the diagnoses, or in recognition of cotemporary labors.

The second part, which has come to our hands through the kind attention of Prof. Jan, treats of the *Calamaridæ*. It is not our intention to discuss the classification of the suborder of the *Asinea*,[1] but we will remark that we doubt whether any herpetologist can characterize with precision more than three subordinate groups—viz., *Boidæ*, *Achrochordidæ* and *Colubridæ*.[2] The subdivisions of the last are so completely interwoven and gradually connected, that no author has yet presented us with characters by which we can isolate them in a natural manner. It has therefore seemed best that the term "family" should be restricted to the three groups here mentioned. It is true that among *Colubridæ* the types are as varied as are the relations of these "families," and it may be said that the simplicity of ophidian structure has deprived us of the means of defining groups, whose equivalents are elsewhere much more tangible. Admitting this to be the case,—how nearly equivalent are zoological groups anywhere, and how uniform is zoological rank? Until it can be shown that this rank is not to be expressed by the formula x^{n-1}, we are justified in retaining the varied divisions of *Colubridæ* as *subfamilies*, and in calling *Achrochordus* the type of a *family*, though it exhibit but a little greater degree of differentiation than some of the former.

[1] Eurystomatous serpents with an unabbreviated os maxillare. The other suborders of the Ophidia, as accepted by the writer, are, on the one hand, the Proteroglypha and Salenoglypha, and on the other, Tortricina (Tortricidæ and Uropeltidæ) and Scolecophidia (Typhlopidæ).

[2] Formerly called subfamilies by the writer.

Prof. Jan adopts the name *Calamaridæ*, after Günther, and includes twenty-three genera. As the work is not intended to be a general ophiology, many genera as well as species are omitted. With great propriety he places here the *Platypteryx* and *Stenognathus* of Duméril, which are associated with *Dipsas* in the *Erpétologie Générale*. He adds to those previously known the genera *Pseudorhabdium* (near *Calamaria*), *Adelphicus* (near *Rhabdosoma*), and *Elapotinus* (near *Elapomorphus*), all with ungrooved teeth. He describes twenty-one species, which are new to herpetology. A few others, presumed to be new, have really been previously described, which is not a matter of surprise when we consider the scattered condition of herpetological literature.

Prof. Jan separates from *Rhabdosoma* those Mexican species which possess two pairs of genëal plates, which is probably a judicious change. If the *Cutostoma chalybeum* of Wagler belongs to this group, that author's name will pertain to it rather than to *Rhabdosoma*, as has been urged.[3] In the work before us, however, it is referred to *Elapoides* of Boie, a genus with keeled scales. If Wagler's statement, "squamæ lævissimæ," is correct of the *chalybæum* as it is of the *semidoliatum* (though Günther says "scales keeled"[4]), this reference can hardly be accepted—still less that of *Colobognathus* of Peters, which is abundantly characterized by its deficient dentition and want of temporal plates.

The genus called *Carphophis*, which follows *Elapoides*, was first established under the name *Carphophiops* in d'Orbigny's *Dict. Univ. d'Hist. Nat.*, on the *Coluber amœnus* of Say. This species was called *vermiformis*, while the name *amœnus* was retained for that since called *Helenæ* by Kennicott. Under the impression that the two represented distinct genera, the *Helenæ* was called *Carphophis* in the same work, and following on the same page. As the true application of the name *vermiformis* could not have been ascertained at the time of its publication, *Carphophis* must be retained, though erroneously characterized, and established upon a species different from the afterwards accepted type.

Prof. Jan is in error in identifying *Virginia Harperti*[5] with the *V. Valeriæ* on p. 24. He also employs the name *Conocephalus* for the genus *Haldea*, which we have shown to be inapplicable.[6] So *Ninia* is the older name for *Streptophorus*, and should be employed in its stead. *Aspidura carinata* (p. 29) is the *Haplocercus Ceylonensis* of Günther, published in 1858. *Elapops Petersi* is *E. plumbeater*[7] of three years earlier date. In the genus *Homalosoma* we find *Contia* of Baird and Girard included. I have already alluded to the range of this genus over both continents;[8] it embraces in the Old World the *coronelloides* and *melanocephala* of Prof. Jan's enumeration, with the *Coronella modesta* of Martin. *Psilosoma* Jan, will probably be accepted as a well established genus.

The genus *Elapomorphus* has received many accessions, within a few years, through the labors of Duméril, Günther, Reinhardt and Peters. As adopted in the Prodromus, it embraces four or five distinct genera. Prof. Duméril early[9] alluded to the very peculiar dentition of his *E. Ga-*

[3] Monatsberichte Preuss. Acad. 1859, p. 275; Pr. A. N. S. Phila., 1860, p. 339.
[4] Günther, Proc. Z. S. Lond., 1860, June.
[5] Pr. A. N. S. Phil., 1862, p. 249.
[6] Pr. A. N. S. Phil., 1860, p. 76.
[7] Loc. cit., 1860, p. 566.
[8] Loc. cit., 1862, p. 339.
[9] Rev. Mag. Zool., 1856, p. 468.

bonensis, and he has since made it the type of a genus *Miodon*, which was anticipated by *Urobelus* of Reinhardt. The very anterior position of the grooved tooth, which has but three solid ones in front of it, suggests the yet undiscovered point of transition from *Asinea* to the *Proteroglypha*. *Microsoma* Jan, is an *Elapid* of the same region, possessing many of the peculiarities of *Urobelus*.

There are probably three, certainly two, genera of this group in South America besides Jan's *Elapomojus;* they are *Apostolepis*, *Elapomorphus verus*, and *Phalotris*,[10] corresponding to the sections marked by asterisks in the table on p. 42.

Homalocranium was referred to *Tantilla* of Bd. & Girard on the ground of priority of the latter in 1861;[11] of this, Prof. Jan does not seem to be aware. He describes an *H. Wagneri*, said to have been brought from Florida, which probably does not belong to the genus on account of its entire anal scutum. *Elapotinus*, described as new, and allied to *Elapomorphus*, is also near to *Tantilla* as far as the characters given enable us to decide. The posterior superior maxillaries are not grooved; if other differences exist, we are not informed of them.

Under the head of *Probletorhinidæ*, Prof. Jan unites a number of genera of singular aspect, and undoubted affinity, which mostly inhabit Mexico and Southwestern United States. We have already recognized this group and published a table of the genera in the *Philadelphia Proceedings* for 1861, (p. 302) and are much gratified at this confirmation of the view there expressed. We will now give an artificial synopsis of this group, with the additions and modifications which new material suggests.[12] The African *Ligonirostra* (*Temnorhynchus* Smith, preoccupied in Coleoptera), must be placed near *Prosymna* Gray. *Chilorhina* De Fil. is *Sympholis* Cope, of prior date. Dr. Jan's demonstration of the position of *Ficimia* Gray is as interesting as unexpected; *Amblymetopon* of Günther has never been properly separated from it, if a difference exists. We will accept for the present *Exorhina* Jan, but his *Oxyrhina* is *Chionactis* Cope (long ago characterized by Hallowell), and *Achirhina* is *Toluca* Kenn., also of prior date. Then there must be added *Conopsis*, and perhaps *Brachyurophis* of Günther,[13] *Sonora* Baird & Girard, and *Gyalopium* and *Chilomeniscus* of the writer.

I. *Internasal plates wanting.*

Rostral plate in contact with frontal,	*Ficimia.*
Rostral plate not in contact with frontal,	
Loreal none, anal divided,	*Conopsis.*
Loreal present,	
Anal divided, nasal separate,	*Exorhina.*
Anal entire, nasal confluent with first upper labial,	*Sympholis.*

II. *Internasals confluent with nasals.*

Dentition glyphodont,	*Stenorhina.*
Dentition isodont, muzzle shovel-like,	*Chilomeniscus.*

[10] Pr. A. N. S. Phil., 1861, p. 524. [11] L. c., p. 74.

[12] Farther accessions of material will probably suggest the union of some of the genera.

[13] Ann. Mag. Nat. Hist. 1863, p. 21.

III. *Internasals separate from nasals.*

Two internasals.	
Two prefrontals.	
Nasal and first superior labial confluent; rostral recurved,	*Gyalopium.*
Nasal and labial separate.	
No loreal, one nasal.	
Dentition glyphodont,	*Brachyurophis.*
Dentition isodont,	*Toluca.*
Loreal present.	
Two nasals,	*Sonora.*
One nasal,	*Chionactis.*
One prefrontal,	*Ligonirostra.*
One internasal, one prefrontal,	*Prosymna.*

We have, on a former occasion,[14] alluded to the close connection of this group with the *Coronellinæ*, through *Cemophora* and allied forms; our author perceives exactly the same affinity, but renames the genus just mentioned, *Stasiotes*. *Ficimia* and, as Günther remarks, *Brachyurophis*, are probably related to *Rhinostoma;* the latter is no doubt connected to the beautiful *Heterodon semicinctus*, by Dr. Peters' *Simophis*. *Heterodon d' Orbignyi* connects the red-ringed species with our northern type; thus we are led from *Sonora semiannulata* to *Heterodon platyrhinus!* Ours is indeed no "Ariadne's thread" if we are led to such results. But we have perhaps only lost the clue.

We have only to remark, regarding Prof. Jan's species of this group, that *Chilorhina Villarsii* is *Sympholis lippiens*, and that *Stenorhina quinquelineata* is not a variety of, but a very distinct species from, the *ventralis*—or *Degenhardtii*, as Jan agrees with Peters in calling it.

Thus it appears that Prof. Jan's work, like that of most others, is not free from oversights, many of which are not so excusable as some, which may have been occasioned by nearly simultaneous publications.

14. *Note on the "Glass Coral" of Japan*, (in a letter to Prof. Silliman, Jr., from William Stimpson, dated Smithsonian Institution, Feb. 6, 1863.)

"The 'glass-coral' to which you refer is the *Hyalonema mirabilis* of Gray, which is found in the seas of Japan, and is one of the most beautiful of marine objects. It forms the subject of an elaborate monograph by Brandt, illustrated by four folio plates. This author divides it, on insufficient grounds, into two genera and several species. We have a few specimens in the museum of the Smithsonian Institution. They consist of groups of silicious fibres resembling spun glass, closely wound together in a spiral manner so as to form cylinders of a foot or more in length and the thickness of the little finger. In the centre of the bundle, particularly toward the base, we find a fibrous substance somewhat resembling cotton or rather asbestos, which is composed of very fine fibres, silicious like the larger ones. The cylinders are encrusted by various marine growths, i. e. a Zoanthus, a sponge, and a sea weed;—and to one of them the egg of a shark (*Scyllium?*) was attached by its tendrils. The Zoanthus is so uniformly found upon specimens, and encrusts them so regularly, that both Brandt and Gray do not hesitate to consider the glass-coral as the axis of a polyp related to the Gorgoniæ. Leuckart combats this idea and considers the polyp to be a parasite, while the

[14] Proc. Acad. Phil., 1860, 241.

silicious fibres themselves belong to a sponge allied to the curious *Euplectella* figured by Prof. Owen in the *Transactions of the Linnæan Society*, vol. xxii, pl. xxi. An examination of these figures and the accompanying description will convince the most skeptical of the correctness of Leuckart's view. The cotton-like substance which I have mentioned above as found in the centre of the cylindrical bundles may perhaps give some indications of the character of the sponge to which these curious spiculæ belong, but the sponge usually seen encrusting the bundles is in all probability *not* the true one, as is supposed by some; and the *Zoanthus*, we cannot even consider as a parasite. For, in the Smithsonian specimens, to which my attention was first called by Mr. Verrill, we see abundant evidence that these so-called parasitic growths are the result of the ingenuity of the Japanese curiosity-mongers from whom the specimens are obtained, of which ingenuity we have also instances in the "mermaids" and other artifacts brought from Japan. In the first place the unnatural grouping of the bundles, figured by Brandt and seen in one of the Smithsonian specimens, is effected by means of a gum resembling gum tragacanth. The group thus formed is inserted into a crevice of a fragment of coral so nicely as to have the appearance of growing attached. Some agglutinating substance is also used for the attachment of the zoanthoid polyp, the sponge, and the sea-weed, for upon scraping these off at various points, we found *beneath* each of them *silken threads tied around the bundle of fibres for the purpose of keeping them together!* The nicety with which this is done is wonderful, and the deception is perfect. We should judge that the Japanese must have considerable knowledge of the lower animals, to be able to produce factitious congeries, so nearly agreeing with nature and so well calculated to deceive even practiced naturalists."

[The numerous auditories who have listened to the instructive and entertaining lectures of Dr. Macgowan upon Japan will be amused to learn that the curious glassy zoophyte which the Doctor exhibited turns out (quite unconsciously to the learned lecturer) to be another proof of the skill of that cunning people in manufacturing factitious objects in natural history so curiously as to deceive even skillful naturalists.

The genus *Hyalonema* and the species *H. Sieboldi* will be found described in Dana's Zoophytes, pp. 641, 642. The glassy fibres of *H. mirabilis*, when heated alone in a glass tube, decrepitate and fly into numerous minute spiculæ, splitting longitudinally, and emitting an animal odor without becoming colored. These fibres are pure silica. They do not gelatinize in hot chlorhydric acid, and no trace of lime could be detected in them by the spectroscope. They polarize light only very imperfectly, either in cross section or transversely. The rings of growth are beautifully seen in section, and as many as seventy can be counted arranged about a center which is excentric to the cylinder.—s.]

15. *Prodromus of the History, Structure, and Physiology of the order Lucernariæ;* by Prof. HENRY JAMES CLARK, of Harvard University, Cambridge, Mass. (*Journal of the Boston Society of Natural History*, March, 1863, pp. 531–567).—In the earlier pages of this number of the Journal, (p. 346, article XXXIV,) it may be seen that the Lucernarians are

ranked as a distinct order of Acalephæ. The principal feature in the prodromus, to which we would here call attention, is the division of the order Lucernariæ into two families. The first family, *Cleistocarpidæ* includes such Lucernarians as the genera *Carduella*, *Depastrum*, &c.; and the second family, *Eleutherocarpidæ*, embraces the earliest described *Lucernariæ*, *L. quadricornis*, *L. octoradiata*, *L. campanulata*, &c., which are divided into three genera. Out of the eleven species of the order, the author has collected himself, or obtained from other sources, no less than eight, leaving only three, of which one is doubtful, to be added to his collection. The author's description, of the above-mentioned eight species, shows that he has studied them with the closest anatomical detail, and has drawn up the diagnoses as much from the internal as from the external characters; in fact we should say that the whole structure of these animals is epitomized in the prodromus. The geographical distribution of three of these species,—*Manania auricula* (*Lucernaria auricula* Fabricius), *Haliclystus auricula* (*L. auricula* Rathke, non Fabricius) and *L. quadricornis*,—is interesting, from the fact that they are common to the shores of Europe and America; and we are led to believe that more of the others, which are of the rarest sort, will be yet found to extend across the Atlantic. This view is in accordance with the opinions of most of the leading zoologists, both of this country and Europe, in regard to the other animals of the North Atlantic fauna. Of the rest of the eleven species comprised in the order, five are European and two are American. The latter two are entirely new to science.

VII. ASTRONOMY AND METEOROLOGY.

1. *Re-discovery of Panopea, Asteroid* ⑦⑨.—Panopea was re-discovered by R. Luther at Bilk, Oct. 21, 1862. According to an observation of Oct. 28th, the error of Duner's ephemeris was $-8^m\ 13^s$ in A. R and $-1°\ 19'$ in Dec.

2. *Elements of Asteroid* ⑦⑨.—The following elements of Freya, Asteroid ⑦⑨, have been computed by D'Arrest of Copenhagen.

Epoch 1862, Oct. 24·5, Greenwich m. t.

M	=	321° 37′ 44″·94	
π	=	67 10 17 ·9	Mean equinox 1862·0.
Ω	=	212 29 32 ·5	
i	=	2 13 3 ·0	
φ	=	1 43 52 ·4	
log. a	=	0·503649	
μ	=	623·066	

3. *Discovery of Asteroid* ⑦⑦.—On the 12th of November, 1862, a new planet was discovered by Dr. C. H. F. Peters, at Hamilton College Observatory. It was near Feronia, and was between the 11th and 12th magnitudes.

4. *Comet* III, 1862.—This comet was discovered on the morning of Nov. 28, by Professor Respighi, at Bologna, and three days later by Dr. Bruhns at Leipsic. The following elements have been communicated by R. Engelmann of Leipsic.

T	=	1862, Dec. 28·18262.
π	=	125° 9′ 42″·6
☊	=	355 44 57 ·9
i	=	42 22 52 ·5
log. q	=	9·904475

Motion retrograde.

5. *Comet* I, 1863.—This comet was discovered by Dr. Bruhns of Leipsic, on the morning of Dec. 2d. The following elements have been computed by F. Tiejen of Berlin.

T	=	1863, Feb. 3·52928 Berlin m. t.	
π	=	191° 23′ 12″·2	Apparent equinox, 1862, Dec. 6·0.
☊	=	116 55 28 ·0	
i	=	85 21 42 ·8	
log. q	=	9·9002165	

Motion direct.

6. *Star Shower in December*, 1565.—In a *Sagenbuch der Lausitz* by Karl Haupt, published in the *Neues Lausitzisches Magazin* (Görlitz, 1862), among *Wunderzeichen am Himmel*, gathered from old Lusatian chronicles, is the following:

"On the 3d of December, 1565, there fell at Sorau fire from heaven like flakes of snow."—*Magnus*, (*Joh. Lam.*) *Historische Beschreibung von Sorau.* Leipz., 1710. 4to.

7. *Shooting Stars seen in England in* 1862.—The usual displays of shooting stars this year (1862), as seen near Manchester, have not been as well marked as usual; that of August 10th–11th, perhaps less so than for the last few years, but the weather was not very favorable. That of Nov. 9th–10th[1] was not in the least marked, either as regards the numbers or radiant. But the more newly determined period for Dec. 10th–12th has been exceedingly well defined, and the radiant point, both for the last year and for the present one, perfectly referable to a part of the heavens halfway between β Aurigæ and α Geminorum.—*R. P. Greg in Phil. Mag.*

8. *Auroral arch of April* 9th, 1863.—On the evening of April 9th, there was noticed at New Haven some appearance of an auroral light between 7½ and 8 o'clock. About 9 o'clock, white columns rose both from the eastern and western horizon, and shot up towards the meridian, their tops inclining from a vertical direction about fifteen degrees towards the south. A line of shorter columns connected the two columns just mentioned, in such a manner as to form a tolerably regular arch, spanning the heavens, and passing exactly over the Dipper. This arch was evidently formed of short streamers parallel to each other. Most of them were from 10° to 15° in length, and for some time presented the appearance of a row of comet's tails all parallel to each other. By 9½ o'clock, the eastern portion had very much faded, but the column in the west was intensely bright, and of a white color. It extended to Castor, and passed centrally over a star about midway between the two horns of the Bull. At 10 o'clock, the column in the east had disappeared entirely, while that in the west had very much faded, but extended up nearly or quite to the meridian. During the entire evening, there was noticed a

[1] This, it will be observed, is not the proper anniversary of the November shower.

very strong auroral glow above the northern horizon, with the usual dark segment beneath it.

The following notice of this aurora appeared in the *Newburyport Daily Herald* of April 10th, with the signature P., presumed to denote Dr. Henry C. Perkins.

"An auroral arch of intense brightness spanned the heavens last evening as the bell was ringing for 9 o'clock, equalled only during our remembrance by that of Aug., 1827. Starting from a point just above the horizon, not far from due East, it enveloped the star in the right knee of Bootes, passed amid the stars in the Sickle, or the head and neck of Leo, thence enveloping Castor and Pollux, covering the space between the feet of the Twins, swerving thence a little to the northwest between the heads of Orion and Taurus, until it faded from view.

The arch was about 8° in width, remarkably well defined, though not so sharply so as that in 1827. It gradually moved to the South at the rate of about 8° or 10° in 20 minutes, when it broke up into wisps of light strikingly resembling those seen in the tail of Donati's comet, and so beautifully and truly represented in Prof. Bond's drawing of that body.—P."

[Correspondents are requested to send in their observations on this auroral arch, which was probably seen over sufficient area to furnish data for approximate estimates of its height.]

VIII. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *National Academy of Sciences.*—Hon. Henry Wilson of Massachusetts, in the Senate of the United States, at the last session of Congress, brought forward and secured the unanimous passage of the following bill entitled,

"A Bill to incorporate the National Academy of Sciences.

"*Be it enacted by the Senate and House of Representatives of the United States of America in Congress assembled*, That LOUIS AGASSIZ, Massachusetts; J. H. ALEXANDER, Maryland; S. ALEXANDER, New Jersey; A. D. BACHE, at large; F. A. P. BARNARD, at large; J. G. BARNARD, United States army, Massachusetts; W. H. C. BARTLETT, United States Mil. Acad., Missouri; U. A. BOYDEN, Massachusetts; ALEXIS CASWELL, Rhode Island; WILLIAM CHAUVENET, Missouri; J. H. C. COFFIN, United States Naval Academy, Maine; J. A. DAHLGREN, United States navy, Pennsylvania; J. D. DANA, Connecticut; CHARLES H. DAVIS, United States navy, Massachusetts; GEORGE ENGELMANN, St. Louis, Missouri; J. F. FRAZER, Pennsylvania; WOLCOTT GIBBS, New York; J. M. GILLISS, United States Naval Observatory, Kentucky; A. A. GOULD, Massachusetts; B. A. GOULD, Massachusetts; ASA GRAY, Massachusetts; A. GUYOT, New Jersey; JAMES HALL, New York; JOSEPH HENRY, at large; J. E. HILGARD, at large, Illinois; EDWARD HITCHCOCK, Massachusetts; J. S. HUBBARD, United States naval observatory, Connecticut; A. A. HUMPHEYS, United States army, Pennsylvania; J. L. LECONTE, United States army, Pennsylvania; J. LIEDY, Pennsylvania; J. P. LESLEY, Pennsylvania; M. F. LONGSTRETH, Pennsylvania; D. H. MAHAN, United States Military Academy, Virginia; J. S. NEWBERRY, Ohio; H. A. NEWTON, Connecticut; BENJAMIN PEIRCE, Massachusetts; JOHN RODGERS, United States navy, Indiana; FAIRMAN ROGERS, Pennsylvania; R. E. ROGERS, Pennsylvania; W. B. ROGERS, Massachusetts; L. M. RUTHERFURD, New York; JOSEPH SAXTON, at large; BENJAMIN SILLIMAN, Connecticut; BENJAMIN SILLIMAN, Jr., Connecticut; THEODORE STRONG, New Jersey; JOHN TORREY, New York; J. G. TOTTEN, United States army, Connecticut; JOSEPH WINLOCK, United States Nautical Almanac, Kentucky; JEFFRIES WYMAN, Massachusetts; J. D. WHITNEY, California, their associates and successors duly chosen, are hereby incorporated, constituted, and declared to be a body corporate, by the name of the National Academy of Sciences.

"SEC. 2. *And be it further enacted*, That the National Academy of Sciences shall consist of not more than fifty ordinary members, and the said corporation hereby constituted shall have power to make its own organization, including its constitution, by-laws, and rules and regulations; to fill all vacancies created by death, resig-

nation, or otherwise; to provide for the election of foreign and domestic members, the division into classes, and all other matters needful or usual in such institution, and to report the same to Congress.

"SEC. 3. *And be it further enacted*, That the National Academy of Sciences shall hold an annual meeting at such place in the United States as may be designated, and the Academy shall, whenever called upon by any department of the government, investigate, examine, experiment, and report upon any subject of science or art, the actual expense of such investigations, examinations, experiments, and reports, to be paid from appropriations which may be made for the purpose, but the Academy shall receive no compensation whatever for any services to the government of the United States."

Agreeably to an invitation from Mr. Wilson, a majority of the corporators named in this Act met on the 22d of April, at 11 A. M. in the Chapel of the University of the city of New York, for the purpose of organizing the National Academy of Sciences. The body was called to order, with a few appropriate remarks, by Mr. Wilson, who was present by the request of a large number of members. A temporary organization was secured by the choice of Joseph Henry of Washington and Alexis Caswell of Brown University as Chairman and Secretary, *pro tempore*. A committee of nine persons, of whom Prof. Caswell was chairman, was appointed to prepare and report Rules for governing the Academy, agreeably to the powers vested in them by Section 2d of the Act of Incorporation, adopted by Congress and approved by the President of the United States on the 4th of March, 1863.

Agreeably to the laws thus enacted (which lay over to January next for final consideration), the Academy is divided into two classes, viz.:[1]

1st. The Class of Mathematics and Physics.

2d. The Class of Natural History.

The corporate members elect under which of these two classes and in which section of that class they will inscribe their names. The classes are subdivided thus:—

A. *Class of Mathematics and Physics.*—SECTIONS: 1, Mathematics; 2, Physics; 3, Astronomy, Geography, and Geodesy; 4, Mechanics; 5, Chemistry.

B. *Class of Natural History.*—SECTIONS: 1, Mineralogy and Geology; 2, Zoology; 3, Botany; 4, Anatomy and Physiology; 5, Ethnology.

While each member chooses his own position, he may also be elected an honorary member of any section by the members thereof, and the Academy retains the power of transferring a member from one Section to another.

There may be fifty foreign associates, who take no part in the business of the Academy, but have the privilege of attending its sessions, reading and communicating papers and of receiving a copy of the publications of the Academy.

The officers of the Academy are a President, a vice President, a Foreign Secretary, a home Secretary, and a Treasurer, all of whom are elected for a term of six years.

There is also a chairman and secretary to each class elected annually at each January meeting. The officers of the Academy, and chairman of the classes, together with four members to be annually elected by the Academy, constitute a Council for the transaction of such business as is assigned to them by law or by the Academy.

[1] As these Rules are subject to change prior to their final adoption in January, there is an obvious impropriety in publishing them in detail, at present, but so much of their provisions as concern the general organization of the Academy, and as are not likely to be materially altered, we give in this notice.—EDS.

The powers of the President, (or in case of his absence or disability, the Vice-President,) are, to preside at the meetings of the Academy, name (unless otherwise provided for by law) committees of members, referring business, experimental enquiries, investigations or preliminary inquiries required by the Government of the United States or its branches, to members specially conversant with the subject; and, with the Council, to direct the general business of the Academy. The duties of the other officers present nothing beyond what is usual in all similar organizations.

The Academy holds two stated meetings in each year, one in January and one in August. The January meeting is to be held always in Washington on the 3d day of January, (or when that day is a Sunday, on the 4th), but the August meeting will be held at such place as the Academy at any previous meeting may designate, and on the 3d Wednesday of the month. The scientific meetings of the Academy are to be open or public, the business meetings closed. Communications by persons not members of the Academy are to be presented and read by a member who makes himself responsible only for the general propriety of the paper and not for opinions expressed by the author.

Propositions for researches, experiments, observations, investigations or reports, shall originate with the Classes to which the subjects are appropriate, and then be submitted to the Academy for discussion, and approval or rejection, excepting propositions from the Government of the United States, or any of its branches, which shall be acted on by the President, who will in such case report, if necessary, at once to the Government, and also to the Academy at the next stated meeting. The judgment of the Academy is to be at all times at the disposition of the Government upon any matter of Science or Art within the limits of the subjects embraced by it. The President of the Academy is competent, in special cases, to call in the aid, upon committees, of experts, or men of remarkable attainments not members of the Academy.

The Annual Report to be presented to both Houses of Congress, is to be prepared by the President of the Academy, and before its presentation is to be submitted, first to the Council, and then to the Academy at the January meeting. The abstract of a memoir may however be sent by any member to the Home Secretary, to be printed and circulated among the members during the recess of the Academy.

These are the most important features of the organic law of the National Academy of Sciences. An election was held under the rules when the following officers were chosen almost unanimously:

President,	ALEXANDER DALLAS BACHE,	Washington, D. C.
Vice-President,	JAMES D. DANA,	New Haven, Conn.
Foreign Secretary,	LOUIS AGASSIZ,	Cambridge, Mass.
Home Secretary,	WOLCOTT GIBBS,	New York.
Treasurer,	FAIRMAN ROGERS,	Philadelphia.

OFFICERS OF THE CLASSES.

Class A. *Mathematics and Physics.*

Chairman,	B. PEIRCE,	Cambridge, Mass.
Secretary,	B. A. GOULD,	Cambridge, "

Class B. *Natural History.*

Chairman,	B. SILLIMAN,	New Haven, Ct.
Secretary,	J. S. NEWBERRY,	Ohio.

After the completion of the organization, each member present, agreeably to the requirements of the organic law, took the oath of allegiance prescribed by the Senate of the United States for its own members, and in addition thereto took an oath faithfully to discharge the duties of a member of the National Academy of Sciences to the best of his ability.

Born in the midst of a great political revolution, the National Academy of Sciences, created by the supreme law of the land, stands pledged to the power which has called it into being, and to the world to discharge its duties with fidelity. The members of the Academy named in the Act had before them simply to accept or to decline the trust reposed in them, by no choice of theirs. So far as they have accepted their position, we feel justified in saying it is with a conviction that there were many not named on the list who might most properly have been there, and with the assurance that so far as any honor may attach to membership, it will be shared much more largely by those who shall hereafter be called by the suffrages of the Academy to fill such vacancies as must occur, than by the corporators who are named in the law.

The National Academy of Sciences does not take the place of, or necessarily interfere with, the American Association for the Advancement of Science, as many persons seem to have supposed.

IX. BOOK NOTICES.

1. *The National Almanac and Annual Record for the year* **1863.** Philadelphia: G. W. Childs, 1863. 12mo. pp. 698.—This work succeeds the well known *American Almanac* so long issued at Boston, and which attained an enviable reputation as a reliable record of cosmical facts and the repository of a large amount of information, statistical, political, educational, scientific and general. Mr. Childs has taken up the task relinquished by Mr. Sawyer, and with a degree of fullness surpassing the original. The articles of scientific value in this volume are Meteorology, Tide tables for Coast of U. S., Coast Survey, and Smithsonian Institution, which are excellent. In the same category should be mentioned a valuable paper on the changes in the relative position (in population and growth) of the several States from 1790 to 1860, by Prof. Wm. M. Gillespie, who has exhibited this subject graphically by the method so common in tabulating scientific results by curves. With the active aid of Profs. Bache and Henry of Washington, Profs. Coppée and Gillespie, Dr. Pollock and others, the reliable character of the *National Almanac* is secured, while, as we are informed, the spirited editor retains the best talent, in all departments, before given to the *American Almanac.*

2. *The Geological Evidences of the Antiquity of Man*, with remarks on "theories of the origin of species by variation;" by Sir Charles Lyell, F.R.S. pp. 520, 8vo. London. Reprinted by G. W. Childs, Philadelphia.

3. Abbé Moigno's new Journal, called *Les Mondes Revue Hebdomadaire des Sciences, et les applications aux arts et a l'industrie*, appeared first on the 12th of February of this year, and has reached us regularly each week since. It replaces *Cosmos*, from which Abbé Moigno has withdrawn, for reasons of a personal nature growing out of his relations to the proprietor, which he fully sets forth in a *Prologue* of 6 or 7 pages.

Les Mondes is an extremely lively Journal of 28 pages, with a supplement on pure Science of 16 pages each week. It aims to notice the progress of all science, pure and applied, whether French or foreign.

Index photographed at the
beginning for the convenience
of the microfilm user.